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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

H. H. TURNER, M.A., B.Sc., F.R.S., Sec. R.A.S.,
SAVILIAN PROFESSOR,
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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1897.

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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 248. 1897. Vol. XX.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations and phenomena of Jupiter's Satellites are from the 'Nautical Almanac'; the diagrams and Ephemerides of the Satellites of the other planets from the 'American Ephemeris.' The Ephemeris of Jupiter's 5th satellite is taken from R. A. S. Mon. Not. vol. lvi. p. 542, where Mr. Marth made the following remark:—"As no measurements of the fifth satellite made during the last season have yet come to my knowledge, I must content myself with going on with the adopted daily rate of motion 722°·633 deduced from the measures of 1892 and 1893, so that its uncertainty will now appear trebled."

It has seemed advisable to give particulars of an Eclipse of the year 1898 which occurs early in that year.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used in all cases, and the astronomical day is reckoned from noon to noon as in previous years.

Date.	Day of Year.	Fr. of Year.	Sun		☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			Sets.	Rises.				
			h m	h m	° ′	h m s	h m s	d h m
Jan. 3	3	·006	4 2	20 8	22 47 S	0 4 57	18 53 28	● 2 18 3
10	10	·025	4 10	20 5	21 53	7 58	19 21 4	☽ 10 9 46
17	17	·044	4 21	19 59	20 38	10 29	19 48 40	○ 18 8 17
24	24	·063	4 33	19 50	19 4	12 26	20 16 16	☾ 25 8 9
31	31	·082	4 45	19 40	17 13	13 44	20 43 52	
Feb. 7	38	·101	4 58	19 29	15 8	14 23	21 11 28	● 1 8 13
14	45	·121	5 11	19 16	12 49	14 22	21 39 4	☽ 9 7 25
21	52	·140	5 24	19 2	10 21	13 45	22 6 39	○ 16 22 11
28	59	·159	5 36	18 47	7 45	12 37	22 34 15	☾ 23 15 44
Mar. 7	66	·178	5 49	18 31	5 3	11 5	23 1 51	● 2 23 56
14	73	·197	6 1	18 16	2 18 S	9 13	23 29 27	☽ 11 3 28
21	80	·216	6 13	18 0	0 27 N	7 9	23 57 3	○ 18 9 28
28	87	·235	6 24	17 44	3 12	5 1	0 24 39	☾ 25 0 0
April 4	94	·254	6 36	17 28	5 54	2 55	0 52 15	● 1 16 4
11	101	·274	6 48	17 12	8 31	0 57	1 19 51	☽ 9 20 27
18	108	·293	6 59	16 57	11 1	23 59 13	1 47 26	○ 16 18 25
25	115	·312	7 16	16 43	13 22	57 49	2 15 2	☾ 23 9 48
May 2	122	·331	7 22	16 30	15 32	56 49	2 42 38	● 1 8 46
9	129	·350	7 33	16 17	17 30	56 16	3 10 14	☽ 9 9 37
16	136	·370	7 44	16 7	19 14	56 10	3 37 50	○ 16 1 55
23	143	·389	7 54	15 58	20 41	56 32	4 5 26	☾ 22 21 35
30	150	·408	8 3	15 51	21 51	57 20	4 33 2	● 31 0 26
June 6	157	·427	8 10	15 46	22 43	58 28	5 0 38	☽ 7 19 3
13	164	·446	8 15	15 44	23 15	59 51	5 28 14	○ 14 9 2
20	171	·465	8 18	15 45	23 27	0 1 20	5 55 50	☾ 21 11 24
27	178	·485	8 19	15 47	23 19	2 50	6 23 25	● 29 14 55
July 4	185	·504	8 17	15 52	22 51	4 11	6 51 1	☽ 7 1 32
11	192	·523	8 12	15 59	22 3	5 17	7 18 37	○ 13 16 52
18	199	·542	8 6	16 7	20 57	5 59	7 46 13	☾ 21 3 8
25	206	·561	7 57	16 17	19 34	6 16	8 13 49	● 29 3 58
Aug. 1	213	·580	7 46	16 27	17 54	6 5	8 41 25	☽ 5 6 25
8	220	·600	7 34	16 38	16 0	5 24	9 9 1	○ 12 2 23
15	227	·619	7 21	16 49	13 54	4 14	9 36 37	☾ 19 20 29
22	234	·638	7 7	17 0	11 37	2 38	10 4 13	● 27 15 29
29	241	·657	6 52	17 10	9 11	0 42	10 31 49	
Sept. 5	248	·676	6 36	17 22	6 38	23 58 29	10 59 24	☽ 3 11 13
12	255	·695	6 20	17 39	3 59	56 4	11 27 0	○ 10 14 12
19	262	·715	6 4	17 45	1 17 N	53 36	11 54 36	☾ 18 14 51
26	269	·734	5 48	17 56	1 27 S	51 11	12 22 12	● 26 1 46
Oct. 3	276	·753	5 32	18 8	4 10	48 56	12 49 48	☽ 2 17 32
10	283	·772	5 16	18 19	6 51	46 56	13 17 24	○ 10 4 42
17	290	·791	5 1	18 31	9 28	45 20	13 45 0	☾ 18 9 9
24	297	·810	4 47	18 44	11 57	44 14	14 12 36	● 25 11 28
31	304	·830	4 33	18 56	14 18	43 43	14 40 11	
Nov. 7	311	·849	4 21	19 9	16 27	43 50	15 7 47	☽ 1 2 37
14	318	·868	4 10	19 21	18 23	44 38	15 35 23	○ 8 21 50
21	325	·887	4 1	19 33	20 3	46 8	16 2 59	☾ 17 2 2
28	332	·906	3 55	19 43	21 25	48 17	16 30 35	● 23 21 20
								☽ 30 15 15
Dec. 5	339	·925	3 50	19 53	22 27	50 59	16 58 11	○ 8 16 54
12	346	·945	3 49	20 0	23 8	54 7	17 25 47	☾ 16 16 22
19	353	·964	3 50	20 6	23 26	57 31	17 53 23	● 23 7 55
26	360	·983	3 54	20 8	23 21 S	0 1 1	18 20 59	☽ 30 7 27

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	19 56	23 20	27 22 S	−70·1 E.
2	S	sets	……	26 9	−82·3 E.
3	**S**	4 8	0 21	23 18	+85·5 M.
4	M	5 33	1 17	19 12	+73·4 M.
5	Tu	6 56	2 8	14 15	+61·2 M.
6	W	8 13	2 55	8 47	+49·1 M.
7	Th	9 29	3 38	3 8 S	+36·9 M.
8	F	10 39	4 19	2 31 N	+24·8 M.
9	S	11 50	5 0	7 58	+12·6 M.
10	**S**	13 0	5 40	13 4	+ 0·4 M.
11	M	14 12	6 22	17 40	−11·7 M.
12	Tu	15 22	7 7	21 36	−23·9 M.
13	W	16 33	7 54	24 41	−36·0 M.
14	Th	17 41	8 44	26 41	−48·2 M.
15	F	18 38	9 38	27 25	−60·3 M.
16	S	19 23	10 32	26 45	−72·5 M.
17	**S**	19 57	11 27	24 39	−84·7 M.
18	M	rises	12 20	21 10	+83·2 E.
19	Tu	5 22	13 12	16 32	+71·0 E.
20	W	6 44	14 1	10 59	+58·9 E.
21	Th	8 8	14 49	4 51 N	+46·7 E.
22	F	9 32	15 36	1 35 S	+34·6 E.
23	S	10 55	16 24	7 59	+22·4 E.
24	**S**	12 19	17 15	14 0	+10·2 E.
25	M	13 48	18 8	19 17	− 1·9 E.
26	Tu	15 16	19 5	23 30	−14·1 E.
27	W	16 38	20 5	26 18	−26·3 E.
28	Th	17 45	21 7	27 26	−38·4 E.
29	F	18 37	22 7	26 48	−50·6 E.
30	S	19 12	23 4	24 32	−62·7 E.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	4 30	……	16 14 S	−87·1 E.
2	Tu	5 49	0 46	10 55	+80·8 M.
3	W	7 6	1 31	5 15 S	+68·6 M.
4	Th	8 19	2 13	0 29 N	+56·5 M.
5	F	9 31	2 54	6 5	+44·3 M.
6	S	10 43	3 35	11 23	+32·1 M.
7	**S**	11 53	4 17	16 12	+20·0 M.
8	M	13 5	5 0	20 23	+ 7·8 M.
9	Tu	14 17	5 46	23 45	− 4·4 M.
10	W	15 25	6 34	26 8	−16·5 M.
11	Th	16 26	7 26	27 20	−28·7 M.
12	F	17 16	8 19	27 12	−40·9 M.
13	S	17 55	9 14	25 39	−53·0 M.
14	**S**	18 24	10 8	22 42	−65·2 M.
15	M	18 46	11 0	18 27	−77·4 M.
16	Tu	rises	11 51	13 9	−89·5 M.
17	W	5 45	12 41	7 3	+78·3 E.
18	Th	7 10	13 30	0 30 N	+66·1 E.
19	F	8 37	14 19	6 8 S	+54·0 E.
20	S	10 4	15 10	12 27	+41·8 E.
21	**S**	11 32	16 4	18 5	+29·6 E.
22	M	13 2	17 0	22 38	+17·5 E.
23	Tu	14 26	18 0	25 47	+ 5·3 E.
24	W	15 39	19 1	27 18	− 6·9 E.
25	Th	16 34	20 0	27 5	−19·1 E.
26	F	17 14	20 58	25 15	−31·3 E.
27	S	17 41	21 51	22 1	−43·4 E.
28	**S**	18 0	22 40	17 43 S	−55·6 E.

MARCH.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	18 18	23 25	12 40 S	−67·8 E.
2	Tu	sets	……	7 10	−80·0 E.
3	W	6 2	0 8	1 28 S	+87·9 M.
4	Th	7 15	0 50	4 11 N	+75·7 M.
5	F	8 26	1 30	9 37	+63·5 M.
6	S	9 36	2 12	14 36	+51·4 M.
7	**S**	10 48	2 55	19 1	+39·2 M.
8	M	12 0	3 39	22 39	+27·1 M.
9	Tu	13 8	4 27	25 21	+14·9 M.
10	W	14 13	5 16	26 57	+ 2·7 M.
11	Th	15 7	6 8	27 18	− 9·5 M.
12	F	15 50	7 1	26 18	−21·7 M.
13	S	16 22	7 54	23 57	−33·9 M.
14	**S**	16 46	8 47	20 18	−46·1 M.
15	M	17 6	9 38	15 29	−58·2 M.
16	Tu	17 23	10 28	9 43	−70·4 M.
17	W	17 38	11 17	3 18 N	−82·6 M.
18	Th	rises	12 7	3 25 S	+85·2 E.
19	F	7 35	12 59	10 3	+73·0 E.
20	S	9 7	13 53	16 9	+60·8 E.
21	**S**	10 39	14 51	21 14	+48·7 E.
22	M	12 9	15 52	24 55	+36·5 E.
23	Tu	13 28	16 54	26 55	+24·3 E.
24	W	14 31	17 55	27 7	+12·1 E.
25	Th	15 14	18 53	25 38	− 0·1 E.
26	F	15 46	19 48	22 43	−12·3 E.
27	S	16 7	20 37	18 41	−24·5 E.
28	**S**	16 25	21 23	13 52	−36·7 E.
29	M	16 38	22 6	8 33	−48·8 E.
30	Tu	16 51	22 48	2 59 S	−61·0 E.

APRIL.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	17 14	……	8 3 N	−85·4 E.
2	F	sets	0 9	13 9	+82·4 M.
3	S	8 34	0 51	17 42	+70·2 M.
4	**S**	9 46	1 35	21 33	+58·0 M.
5	M	10 55	2 22	24 31	+45·8 M.
6	Tu	12 0	3 10	26 25	+33·6 M.
7	W	12 59	4 1	27 7	+21·4 M.
8	Th	13 44	4 53	26 33	+ 9·2 M.
9	F	14 20	5 45	24 41	− 3·0 M.
10	S	14 47	6 36	21 34	−15·2 M.
11	**S**	15 9	7 26	17 18	−27·4 M.
12	M	15 25	8 15	12 4	−39·6 M.
13	Tu	15 40	9 4	6 3 N	−51·8 M.
14	W	15 55	9 53	0 27 S	−63·9 M.
15	Th	16 11	10 43	7 7	−76·1 M.
16	F	rises	11 37	13 31	−88·3 M.
17	S	8 4	12 34	19 9	+79·5 E.
18	**S**	9 39	13 35	23 31	+67·3 E.
19	M	11 7	14 39	26 13	+55·1 E.
20	Tu	12 18	15 44	27 2	+42·9 E.
21	W	13 12	16 45	26 0	+30·7 E.
22	Th	13 47	17 42	23 22	+18·5 E.
23	F	14 12	18 34	19 31	+ 6·3 E.
24	S	14 31	19 22	14 50	− 6·0 E.
25	**S**	14 45	20 6	9 37	−18·2 E.
26	M	14 59	20 47	4 9 S	−30·4 E.
27	Tu	15 9	21 28	1 24 N	−42·6 E.
28	W	15 22	22 8	6 49	−54·8 E.
29	Th	15 34	22 50	11 56	−67·0 E.
30	F	15 50	22 [illegible]	16 [illegible] N	[illegible]

MAY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	7 35	……	20 35 N	+88·6 M.
2	**S**	8 44	0 19	23 45	+76·4 M.
3	M	9 52	1 7	25 53	+64·2 M.
4	Tu	10 52	1 57	26 52	+52·0 M.
5	W	11 42	2 48	26 36	+39·8 M.
6	Th	12 20	3 40	25 4	+27·6 M.
7	F	12 49	4 30	22 19	+15·4 M.
8	S	13 11	5 20	18 27	+ 3·2 M.
9	**S**	13 29	6 8	13 39	− 9·1 M.
10	M	13 45	6 55	8 5	−21·3 M.
11	Tu	14 0	7 41	1 57 N	−33·5 M.
12	W	14 13	8 30	4 28 S	−45·7 M.
13	Th	14 30	9 20	10 51	−57·9 M.
14	F	14 50	10 15	16 45	−70·1 M.
15	S	15 17	11 14	21 42	−82·3 M.
16	**S**	rises	12 18	25 10	+85·5 E.
17	M	9 55	13 24	26 47	+73·3 E.
18	Tu	10 58	14 29	26 25	+61·1 E.
19	W	11 43	15 30	24 15	+48·8 E.
20	Th	12 14	16 26	20 39	+36·6 E.
21	F	12 34	17 17	16 3	+24·4 E.
22	S	12 52	18 3	10 51	+12·2 E.
23	**S**	13 5	18 46	5 21 S	0·0
24	M	13 18	19 27	0 13 N	−12·3 E.
25	Tu	13 29	20 8	5 41	−24·5 E.
26	W	13 41	20 49	10 51	−36·7 E.
27	Th	13 57	21 31	15 35	−48·9 E.
28	F	14 15	22 16	19 43	−61·1 E.
29	S	14 38	23 4	23 4	−73·3 E.
30	**S**	sets	23 53	25 26	−85·5 E.
[illegible]	M	[illegible]	[illegible]	[illegible]	[illegible]

JUNE.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	9 38	0 44	26 40 N	+70·0 M.
2	W	10 21	1 36	25 23	+57·8 M.
3	Th	10 52	2 27	22 53	+45·6 M.
4	F	11 15	3 17	19 16	+33·4 M.
5	S	11 35	4 5	14 43	+21·1 M.
6	**S**	11 50	4 51	9 25	+ 8·9 M.
7	M	12 5	5 37	3 34 N	− 3·3 M.
8	Tu	12 18	6 23	2 35 S	−15·5 M.
9	W	12 34	7 10	8 47	−27·8 M.
10	Th	12 52	8 1	14 41	−40·0 M.
11	F	13 15	8 56	19 54	−52·2 M.
12	S	13 47	9 57	23 55	−64·4 M.
13	**S**	14 32	11 1	26 18	−76·6 M.
14	M	rises	12 7	26 46	−88·9 M.
15	Tu	9 34	13 12	25 17	+78·9 E.
16	W	10 11	14 12	22 8	+66·7 E.
17	Th	10 37	15 6	17 45	+54·5 E.
18	F	10 56	15 56	12 35	+42·2 E.
19	S	11 10	16 41	7 0	+30·0 E.
20	**S**	11 24	17 24	1 18 S	+17·8 E.
21	M	11 35	18 5	4 18 N	+ 5·6 E.
22	Tu	11 49	18 46	9 37	− 6·7 E.
23	W	12 3	19 29	14 30	−18·9 E.
24	Th	12 19	20 13	18 48	−31·1 E.
25	F	12 41	20 59	22 21	−43·4 E.
26	S	13 9	21 48	24 58	−55·6 E.
27	**S**	13 48	22 39	26 30	−67·8 E.
28	M	14 38	23 31	26 48	−80·0 E.
29	Tu	sets	……	25 48	+87·8 M.
30	W	8 55	0 23	23 33 N	+75·6 M.

JULY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	9 21	1 14	20 8 N	+63·4 M.
2	F	9 42	2 3	15 44	+51·2 M.
3	S	9 58	2 50	10 33	+38·9 M.
4	**S**	10 12	3 35	4 49 N	+26·7 M.
5	M	10 26	4 21	1 14 S	+14·5 M.
6	Tu	10 39	5 7	7 20	+ 2·2 M.
7	W	10 56	5 55	13 12	−10·0 M.
8	Th	11 16	6 47	18 29	−22·2 M.
9	F	11 43	7 43	22 47	−34·4 M.
10	S	12 21	8 44	25 41	−46·6 M.
11	**S**	13 16	9 48	26 51	−58·9 M.
12	M	14 27	10 52	26 8	−71·1 M.
13	Tu	15 53	11 54	23 38	−83·3 M.
14	W	rises	12 52	19 41	+84·5 E.
15	Th	8 59	13 44	14 42	+72·3 E.
16	F	9 16	14 32	9 8	+60·0 E.
17	S	9 30	15 17	3 20 S	+47·8 E.
18	**S**	9 42	16 0	2 27 N	+35·6 E.
19	M	9 55	16 42	7 59	+23·3 E.
20	Tu	10 10	17 24	13 6	+11·1 E.
21	W	10 24	18 8	17 38	− 1·1 E.
22	Th	10 43	18 54	21 26	−13·3 E.
23	F	11 9	19 42	24 21	−25·5 E.
24	S	11 44	20 32	26 13	−37·8 E.
25	**S**	12 29	21 24	26 53	−50·0 E.
26	M	13 28	22 16	26 16	−62·2 E.
27	Tu	14 35	23 8	24 22	−74·4 E.
28	W	15 50	23 58	21 14	−86·6 E.
29	Th	sets	……	17 2	+81·2 M.
30	F	8 4	0 46	11 57	+68·9 M.
31	S	8 19	1 33	6 15 N	+56·7 M.

AUGUST.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	8 34	2 19	0 10 N	+44·5 M.
2	M	8 47	3 5	6 0 S	+32·3 M.
3	Tu	9 3	3 53	11 56	+20·1 M.
4	W	9 22	4 43	17 20	+ 7·8 M.
5	Th	9 46	5 37	21 50	− 4·4 M.
6	F	10 18	6 35	25 4	−16·6 M.
7	S	11 6	7 36	26 43	−28·8 M.
8	**S**	12 8	8 39	26 35	−41·0 M.
9	M	13 27	9 40	24 43	−53·3 M.
10	Tu	14 52	10 39	21 18	−65·5 M.
11	W	16 17	11 33	16 44	−77·7 M.
12	Th	rises	12 22	11 22	−89·9 M.
13	F	7 36	13 9	5 36 S	+77·9 E.
14	S	7 49	13 53	0 16 N	+65·7 E.
15	**S**	8 1	14 35	5 59	+53·5 E.
16	M	8 15	15 18	11 20	+41·3 E.
17	Tu	8 30	16 2	16 8	+29·0 E.
18	W	8 50	16 47	20 13	+16·8 E.
19	Th	9 11	17 34	23 28	+ 4·6 E.
20	F	9 40	18 24	25 41	− 7·6 E.
21	S	10 22	19 15	26 46	−19·8 E.
22	**S**	11 14	20 6	26 36	−32·0 E.
23	M	12 19	20 58	25 9	−42·3 E.
24	Tu	13 31	21 49	22 26	−66·5 E.
25	W	14 49	22 38	18 35	−78·7 E.
26	Th	16 8	23 26	13 45	−80·9 E.
27	F	sets	……	8 9	+86·9 M.
28	S	6 42	0 14	2 4 N	+74·7 M.
29	**S**	6 55	1 1	4 13 S	+62·5 M.
30	M	7 11	1 49	10 23	+50·3 M.
31	Tu	7 29	2 39	16 2 S	+38·1 M.

SEPTEMBER.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	7 52	3 33	20 50 S	+25·9 M.
2	Th	8 22	4 30	24 23	+13·7 M.
3	F	9 3	5 30	26 24	+ 1·4 M.
4	S	10 0	6 32	26 42	−10·8 M.
5	**S**	11 11	7 33	25 17	−23·0 M.
6	M	12 34	8 31	22 21	−35·2 M.
7	Tu	13 57	9 25	18 12	−47·4 M.
8	W	15 19	10 15	13 10	−59·6 M.
9	Th	16 38	11 2	7 36	−71·8 M.
10	F	17 54	11 46	1 49 S	−84·0 M.
11	S	rises	12 30	3 57 N	+83·8 E.
12	**S**	6 22	13 12	9 26	+71·6 E.
13	M	6 36	13 56	14 27	+59·4 E.
14	Tu	6 53	14 41	18 49	+47·2 E.
15	W	7 14	15 27	22 21	+35·0 E.
16	Th	7 41	16 16	24 55	+22·8 E.
17	F	8 17	17 6	26 23	+10·6 E.
18	S	9 4	17 57	26 38	− 1·6 E.
19	**S**	10 3	18 48	25 39	−13·8 E.
20	M	11 11	19 38	23 26	−26·0 E.
21	Tu	12 25	20 28	20 4	−38·2 E.
22	W	13 42	21 16	15 39	−50·4 E.
23	Th	15 3	22 3	10 23	−62·6 E.
24	F	16 23	22 51	4 28 N	−74·8 E.
25	S	17 46	23 40	1 50 S	−87·0 E.
26	**S**	sets	……	8 10	+80·8 M.
27	M	5 34	0 30	14 9	+68·6 M.
28	Tu	5 56	1 24	19 22	+56·4 M.
29	W	6 23	2 22	23 23	+44·3 M.
30	Th	7 1	3 22	25 52 S	+32·1 M.

OCTOBER.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	7 56	4 25	26 35 S	+19·9 M.
2	S	9 4	5 27	25 32	+ 7·7 M.
3	**S**	10 23	6 26	22 57	− 4·5 M.
4	M	11 46	7 21	19 6	−16·7 M.
5	Tu	13 7	8 12	14 21	−28·9 M.
6	W	14 24	8 59	9 1	−41·1 M.
7	Th	15 40	9 43	3 24 S	−53·3 M.
8	F	16 53	10 26	2 17 N	−65·5 M.
9	S	18 6	11 8	7 47	−77·7 M.
10	**S**	rises	11 51	12 53	−89·9 M.
11	M	4 59	12 36	17 26	+78·0 E.
12	Tu	5 19	13 22	21 12	+65·8 E.
13	W	5 44	14 9	24 3	+53·6 E.
14	Th	6 16	14 59	25 50	+41·4 E.
15	F	7 0	15 49	26 27	+29·2 E.
16	S	7 53	16 40	25 51	+17·0 E.
17	**S**	8 56	17 30	24 4	+ 4·9 E.
18	M	10 7	18 18	21 9	− 7·3 E.
19	Tu	11 21	19 6	17 12	−19·5 E.
20	W	12 38	19 52	12 23	−31·7 E.
21	Th	13 55	20 39	6 50	−43·9 E.
22	F	15 18	21 26	0 48 N	−56·0 E.
23	S	16 40	22 16	5 29 S	−68·2 E.
24	**S**	18 8	23 9	11 38	−80·4 E.
25	M	sets	……	17 16	+87·4 M.
26	Tu	4 20	0 6	21 53	+75·2 M.
27	W	4 57	1 7	25 0	+63·0 M.
28	Th	5 45	2 12	26 20	+50·9 M.
29	F	6 50	3 17	25 46	+38·7 M.
30	S	8 10	4 19	23 30	+26·5 M.

NOVEMBER.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	10 55	6 9	15 16 S	+ 2·2 M.
2	Tu	12 15	6 57	10 3	−10·0 M.
3	W	13 29	7 42	4 32 S	−22·1 M.
4	Th	14 43	8 25	1 4 N	−34·3 M.
5	F	15 55	9 7	6 31	−46·5 M.
6	S	17 6	9 49	11 39	−58·6 M.
7	**S**	18 18	10 33	16 17	−70·8 M.
8	M	19 29	11 18	20 13	−83·0 M.
9	Tu	rises	12 5	23 17	+84·8 E.
10	W	4 18	12 54	25 19	+72·6 E.
11	Th	4 57	13 44	26 13	+60·5 E.
12	F	5 48	14 35	25 56	+48·3 E.
13	S	6 48	15 24	24 27	+36·1 E.
14	**S**	7 55	16 13	21 51	+24·0 E.
15	M	9 6	17 0	18 15	+11·8 E.
16	Tu	10 20	17 45	13 48	− 0·4 E.
17	W	11 34	18 30	8 40	−12·6 E.
18	Th	12 52	19 15	2 59 N	−24·8 E.
19	F	14 12	20 2	3 1 S	−36·9 E.
20	S	15 34	20 52	9 5	−49·1 E.
21	**S**	17 2	21 46	14 51	−61·3 E.
22	M	18 34	22 45	19 55	−73·5 E.
23	Tu	20 4	23 49	23 44	−85·6 E.
24	W	sets	……	25 53	+82·3 M.
25	Th	4 30	0 56	26 4	+70·1 M.
26	F	5 46	2 2	24 20	+58·0 M.
27	S	7 11	3 4	21 1	+45·8 M.
28	**S**	8 37	4 1	16 31	+33·7 M.
29	M	10 1	4 52	11 18	+21·5 M.
30	Tu	11 19	5 39	5 44 S	+ 9·7 M.

DECEMBER.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	12 33	6 24	0 5 S	− 2·9 M.
2	Th	13 47	7 6	5 26 N	−15·0 M.
3	F	14 57	7 48	10 37	−27·2 M.
4	S	16 8	8 31	15 20	−39·3 M.
5	**S**	17 19	9 16	19 23	−51·5 M.
6	M	18 28	10 2	22 38	−63·7 M.
7	Tu	19 33	10 50	24 54	−75·8 M.
8	W	rises	11 40	26 4	−88·0 M.
9	Th	3 43	12 31	26 2	+79·9 E.
10	F	4 40	13 21	24 48	+67·7 E.
11	S	5 46	14 10	22 26	+55·5 E.
12	**S**	6 55	14 57	19 4	+43·4 E.
13	M	8 9	15 42	14 50	+31·2 E.
14	Tu	9 24	16 26	9 55	+19·0 E.
15	W	10 36	17 10	4 30 N	+ 6·9 E.
16	Th	11 53	17 55	1 15 S	− 5·3 E.
17	F	13 11	18 41	7 6	−17·5 E.
18	S	14 34	19 31	12 48	−29·6 E.
19	**S**	16 6	20 26	18 0	−41·7 E.
20	M	17 29	21 26	22 16	−53·9 E.
21	Tu	18 53	22 31	25 8	−66·0 E.
22	W	20 4	23 37	26 13	−78·2 E.
23	Th	sets	……	25 20	+89·7 M.
24	F	4 37	0 43	22 36	+77·6 M.
25	S	6 7	1 44	18 26	+65·4 M.
26	**S**	7 34	2 40	13 16	+53·2 M.
27	M	8 57	3 31	7 36	+41·1 M.
28	Tu	10 16	4 18	1 46 S	+28·9 M.
29	W	11 33	5 2	3 57 N	+16·8 M.
30	Th	12 45	5 45	9 21	+ 4·6 M.

List of the principal Meteor-showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α	δ			α	δ	
	°	°			°	c	
Jan. 2 ...	**230**	**+53**	Swift; long paths.	July 28 ...	**339**	**−12**	Slow; long.
3 ...	156	+41	Swift.	30 ...	6	+35	Swift; streaks.
11 ...	220	+13	Swift; streaks.	Aug. 4 ...	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	4-10	350	+51	Swift.
22 ...	208	− 8	Very swift; streaks.	10 ...	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	14 ...	311	+62	Rather swift.
29 ...	213	+52	Very swift.	15 ...	292	+53	Swift; bright.
Feb. 1 ...	211	+69	Slow; short.	16 ...	61	+48	Swift; streaks.
15 ...	236	+11	Swift; streaks.	21 ...	73	+41	Swift; streaks.
15 ...	261	+ 4	Swift; streaks.	22 ...	291	+60	Slow; bright.
20 ...	181	+34	Swift; bright.	25 ...	5	+11	Slow; short.
20 ...	263	+36	Swift; streaks.	Sept. 4 ...	346	± 0	Slow.
Mar. 4 ...	175	+10	Slow; bright.	7 ...	62	+37	Swift; streaks.
14 ...	250	+54	Swift.	15 ...	48	+44	Very swift.
18 ..	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	335	+58	Slowish.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 16 ...	210	−10	Rather swift.	Oct. 2 ...	225	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
19 ...	218	+33	Slow; bright.	4 ...	310	+77	Slowish.
19 ..	229	− 2	Slow; long.	8 ...	77	+31	Swift; streaks.
20 ...	**270**	**+33**	Swift.	11 ...	29	+72	Small; short.
25 ...	272	+21	Swift; short.	14 ...	40	+20	Rather swift.
26 ...	255	+37	Slowish.	14 ...	135	+68	Swift; streaks.
May 1 ...	239	+46	Small; short.	18 ...	**92**	**+15**	Swift; streaks.
5 ...	254	−21	Slowish.	20 ...	106	+12	Swift; streaks.
6 ...	**338**	**− 2**	Swift; streaks.	29 ...	109	+23	Very swift.
7 ...	244	+ 7	Slow; bright.	Nov. 1 ...	43	+22	Slow; bright.
11 ...	231	+27	Slow; small.	2 ...	55	+ 9	Slow; bright.
29 ...	264	+64	Slowish.	7 ..	102	+73	Very swift.
30 ...	333	+27	Swift; streaks.	13-15	**150**	**+22**	Swift; streaks.
June 7 ...	247	−25	Slow; very bright.	16 ...	154	+41	Swift; streaks.
10 ...	261	+ 5	Very slow.	20 ...	62	+23	Slow; bright.
13 ...	310	+61	Swift; streaks.	23-27	**25**	**+44**	Very slow; train
15 ...	291	+52	Swift; small.	30 ...	190	+58	Swift; streaks.
15 ...	285	+23	Slowish.	Dec. 4 ...	162	+58	Swift; streaks.
20 ...	335	+57	Swift.	4 ...	110	+25	Slowish.
26 ...	354	+39	Swift; streaks.	6 ...	80	+23	Slow; bright.
July 4 ...	303	+24	Swift.	8 ...	145	+ 7	Swift; streaks.
7 ...	333	+26	Swift.	8 ...	208	+71	Rather swift.
18 ...	333	+12	Swift.	10 ...	**108**	**+33**	Swift; short.
19 ...	314	+48	Swift; short.	12 ...	119	+29	Rather swift.
22 ...	16	+31	Swift; streaks.	22 ...	194	+67	Swift; streaks.
25 ...	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type.

The Perseids, with max. on August 10, are visible for a considerable period and their radia point exhibits an easterly motion amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α	δ		α	δ		α	δ
	°	°		°	°		°	°
July 19 ...	19	+51	July 29 ...	32	+54	Aug. 8 ...	42	+57
21 ...	22	+52	31 ...	34	+55	10 ...	45	+57
23 ...	25	+52	Aug. 2 ...	36	+55	12 ...	47	+57
25 ...	27	+53	4 ...	38	+56	14 ...	50	+58
27 ...	30	+54	6 ...	40	+56	16 ...	53	+58

1857.	Mercury (☿).						Venus (♀).						Mars (♂).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 2...	20 13 35	21 29 S.	6·2	21 18	1 24	5 30	21 50 24	14 54 S.	16·4	22 14	3 1	7 46	4 43 57	25 21 N.	14·6	1 22	9 53	18 22
12...	20 47 48	17 15	8·0	20 45	1 18	5 49	22 34 35	10 19	17·6	21 55	3 6	8 17	4 38 44	25 12	13·2	0 38	9 8	17 36
22...	20 18 3	16 16	10·0	19 29	0 9	4 46	23 16 21	5 22	19·0	21 31	3 8	8 45	4 39 23	25 9	12·0	23 59	8 30	16 57
Feb. 1...	19 41 50	18 22	9·2	18 22	22 50	3 14	23 55 59	0 15 S.	20·8	21 5	3 8	9 11	4 45 9	25 13	10·8	23 26	7 56	16 24
11...	19 56 25	19 39	7·4	18 11	22 29	2 47	0 33 42	4 50 N.	22·8	20 38	3 6	9 34	4 55 11	25 22	9·8	22 55	7 27	15 57
21...	20 39 37	19 1	6·4	18 12	22 34	2 56	1 9 21	9 42	25·4	20 10	3 3	9 56	5 8 39	25 32	8·8	22 28	7 1	15 32
Mar. 2...	21 28 51	16 36	5·8	18 12	22 49	3 27	1 39 15	13 43	28·4	19 42	2 57	10 12	5 23 4	25 40	8·2	22 7	6 40	15 11
12...	22 29 3	11 54	5·2	18 8	23 10	4 13	2 8 59	17 37	32·4	19 9	2 47	10 25	5 41 11	25 44	7·6	21 46	6 19	14 51
22...	23 33 32	5 7 S.	5·0	17 58	23 35	5 14	2 32 46	20 44	37·6	18 33	2 32	10 31	6 1 0	25 41	7·0	21 27	6 0	14 32
Apr. 1...	0 43 25	3 29 N.	5·0	17 43	0 3	6 25	2 47 11	22 45	44·0	17 54	2 6	10 18	6 22 6	25 28	6·4	21 9	5 41	14 12
11...	1 58 32	12 45	5·4	17 30	0 39	7 50	2 48 8	23 17	51·2	17 11	1 28	9 43	6 44 11	25 4	6·1	20 55	5 24	13 52
21...	3 8 20	19 56	6·6	17 16	1 9	8 3	2 34 1	21 43	57·2	16 28	0 35	8 39	7 6 58	24 27	5·8	20 43	5 7	13 31
May 1...	3 55 22	23 5	8·4	17 1	1 17	9 30	2 12 4	18 13	58·6	15 45	23 28	7 8	7 30 11	23 38	5·4	20 33	4 51	13 9
11...	4 9 5	22 21	10·8	16 41	0 51	8 59	1 55 52	14 18	54·2	15 13	22 33	5 48	7 53 41	22 35	5·2	20 24	4 35	12 46
21...	3 54 0	18 51	12·2	16 2	23 50	7 34	1 53 27	11 45	47·0	14 26	21 53	4 56	8 17 18	21 19	5·0	20 17	4 18	12 18
31...	3 38 32	15 46	11·2	15 30	22 57	6 23	2 4 34	11 2	40·0	14 25	21 26	4 26	8 40 54	19 51	4·8	20 10	4 3	11 56
June 10...	3 47 3	15 52	9·2	15 2	22 29	5 55	2 25 45	11 44	34·2	14 3	21 8	4 12	9 4 27	18 10	4·6	20 6	3 48	11 29
20...	4 23 8	18 42	7·4	14 43	22 28	6 15	2 54 7	13 20	29·6	13 45	20 58	4 11	9 27 51	16 19	4·4	20 2	3 32	11 2
30..	5 25 54	22 17	6·0	14 46	22 54	7 4	3 27 50	15 23	26·0	13 28	20 52	4 16	9 51 8	14 17	4·2	19 57	3 16	10 34
July 10...	6 52 16	23 49	5·2	15 25	23 43	8 5	4 5 44	17 29	23·0	13 14	20 51	4 28	10 14 17	12 5	4·2	19 52	2 59	10 5
20...	8 23 42	21 11	5·0	16 30	0 30	8 32	4 47 7	19 20	20·8	13 4	20 53	4 42	10 37 21	9 46	4·0	19 49	2 43	9 36
30...	9 41 20	15 26	5·2	17 44	1 8	8 34	5 31 25	20 40	19·0	13 0	20 58	4 56	11 0 22	7 21	4·0	19 46	2 27	9 8
Aug. 9...	10 43 12	8 35	5·8	18 43	1 30	8 16	6 17 55	21 19	17·4	13 4	21 6	5 10	11 23 25	4 49	4·0	19 43	2 10	8 57
19...	11 32 12	1 52 N.	6·4	19 28	1 40	7 53	7 5 55	21 7	16·2	13 13	21 14	5 15	11 46 35	2 14 N.	3·8	19 40	1 54	8 8
29...	12 8 12	3 47 S.	7·4	19 51	1 36	7 21	7 54 35	20 0	15·2	13 30	21 24	5 18	12 9 58	0 25 S.	3·8	19 36	1 38	7 40
Sept. 8...	12 24 34	6 56	9·0	19 44	1 13	6 41	8 43 8	17 58	14·4	13 54	21 33	5 12	12 33 39	3 4	3·8	19 33	1 22	7 10
18...	12 8 29	4 57 S.	10·4	18 36	0 18	5 54	9 31 2	15 5	13·6	14 19	21 41	5 3	12 57 45	5 43	3·8	19 32	1 7	6 41
28...	11 38 15	1 25 N.	9·2	16 51	23 3	5 13	10 18 4	11 28	13·0	14 46	21 49	4 52	13 22 23	8 19	3·8	19 30	0 52	6 14
Oct. 8...	11 53 30	2 26 N.	6·8	16 29	22 44	4 59	11 4 13	7 17	12·4	15 15	21 55	4 36	13 47 40	10 52	3·8	19 30	0 38	5 45
18..	12 46 23	2 52 S.	5·4	17 10	22 59	4 50	11 49 49	2 43 N.	12·0	15 44	22 1	4 18	14 13 41	13 18	3·8	19 30	0 25	5 19
28...	13 47 46	9 50	4·8	18 9	23 22	4 36	12 35 19	2 3 S.	11·6	16 13	22 7	4 1	14 40 34	15 36	3·8	19 30	0 12	4 54
Nov. 7...	14 50 4	16 13	4·6	19 7	23 45	4 24	13 21 16	6 49	11·2	16 45	22 14	3 43	15 8 22	17 42	3·8	19 31	0 1	4 31
17...	15 53 41	21 16	4·6	19 59	0 6	4 14	14 8 17	11 22	11·0	17 17	22 22	3 27	15 37 9	19 35	3·8	19 30	23 49	4 7
27...	16 59 29	24 35	4·8	20 49	0 33	4 21	14 56 53	15 29	10·7	17 49	22 31	3 13	16 6 55	21 11	3·8	19 31	23 39	3 47
Dec. 7...	18 6 22	25 45	5·2	21 25	1 0	4 37	15 47 22	18 57	10·4	18 21	22 43	3 4	16 37 37	22 29	3·8	19 31	23 31	3 30
17...	19 7 58	24 31	6·2	21 38	1 23	5 1	16 39 47	21 33	10·2	18 50	22 56	3 2	17 9 10	23 24	3·8	19 30	23 23	3 16
27..	19 42 27	21 33 S.	8·0	21 12	1 17	5 22	17 33 46	23 6 S.	10·2	19 13	23 10	3 7	17 41 25	23 57 S.	3·8	19 27	23 16	3 5

Mercury.

	d h	d h	d h	d h
Gr. elong. E....Jan.	6 7; Apr.	28 3; Aug.	26 5; Dec.	20 12
„ W....Feb.	15 7; June	15 18; Oct.	7 10	
Sup. conj. ......Apr.	1 15; July	15 10; Nov.	7 17	
Inf. conj. ......Jan.	22 2; May	20 19; Sept.	22 0	
Stationary ...Jan.	12 17; Feb.	2 16; May	9 22; June	2 0
„ ...Sept.	8 14; Sept.	30 8; Dec.	27 19	

Venus.

	d h
Gr. elong. E....Feb.	15 10
„ W... July	7 12
Inf. conj. ......Apr.	28 7
Stationary......Apr.	6 16
„ ......May	7 14

Superior Planets.

Conjunction.	Opposition.	Quadrature.		Stationary.	
d h	d h	d h	d h	d h	d h
Mars ...Nov. 21 1	......	Mar. 18 10	......	Jan. 15 19	
Jupiter...Sept. 12 18	Feb. 23 2	May 21 14;	Dec. 30 5	Apr. 26 4	
Saturn...Nov. 24 19	May 17 17	Feb. 18 8;	Aug. 16 15	Mar. 9 15;	July 28 12
Uranus...Nov. 20 20	May 17 6	Feb. 16 20;	Aug. 17 7	Mar. 3 3;	Aug. 2 15
Neptune.June 10 5	Dec. 12 8	Mar. 7 7;	Sept. 14 16	Feb. 25 22;	Sept. 24 19

1897.	Jupiter (♃).						Saturn (♄).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1	10 48 27	8 50 N.	37·8	9 11	16 0	22 47	15 41 39	17 36 S.	14·4	16 22	20 53	1 22
21	10 44 32	9 19	39·8	7 46	14 38	21 28	15 48 43	17 56	14·8	15 12	19 41	0 7
Feb. 10	10 36 41	10 10	41·0	6 14	13 11	20 8	15 53 43	18 8	15·4	14 0	18 28	22 54
Mar. 2	10 26 57	11 9	41·2	4 41	11 43	18 43	15 56 10	18 12	15·8	12 43	17 11	21 37
22	10 18 3	12 0	40·2	3 9	10 15	17 21	15 55 51	18 8	16·4	11 24	15 52	20 18
Apr. 11	10 12 22	12 29	38·6	1 42	8 51	15 58	15 52 54	17 56	16·8	10 2	14 31	18 58
May 1	10 11 10	12 33	36·4	0 21	7 31	14 39	15 47 56	17 39	17·0	8 36	13 7	17 36
21	10 14 33	12 10	34·4	23 8	6 16	13 22	15 41 57	17 20	17·2	7 9	11 42	16 13
June 10	10 21 58	11 25	32·4	22 1	5 5	12 7	15 36 10	17 3	17·0	5 43	10 18	14 51
30	10 32 34	10 21	30·8	20 59	3 57	10 53	15 31 41	16 51	16·6	4 19	8 55	13 30
July 20	10 45 33	9 2	29·6	20 1	2 51	9 39	15 29 21	16 47	16·2	2 58	7 34	12 8
Aug. 9	11 0 9	7 32	28·8	19 4	1 47	8 28	15 29 33	16 53	15·6	1 40	6 16	10 50
29	11 15 42	5 54	28·4	18 10	0 44	7 16	15 32 19	17 8	15·2	0 26	5 0	9 32
Sept. 18	11 31 38	4 12	28·4	17 12	23 38	6 2	15 37 28	17 30	14·8	23 14	3 46	8 16
Oct. 8	11 47 24	2 32	28·6	16 18	22 35	4 50	15 44 35	17 58	14·4	22 6	2 35	7 2
28	12 2 23	0 57 N.	29·2	15 21	21 31	3 39	15 53 11	18 28	14·2	20 59	1 25	5 49
Nov. 17	12 15 54	0 28 S.	30·4	14 23	20 26	2 27	16 2 42	18 57	14·0	19 52	0 15	4 36
Dec. 7	12 27 11	1 37	31·8	13 21	19 18	1 13	16 12 32	19 25	14·0	18 42	23 3	3 24
27	12 35 23	2 24 S.	33·6	12 17	18 8	0 0	16 22 3	19 48 S.	14·2	17 36	21 54	2 10

	Uranus (♅).						Neptune (♆).					
Jan. 1	14 50 52	19 23 S.	3·5	16 31	20 52	1 11	5 9 38	21 30 N.	2·7	2 17	10 22	18 25
31	15 45 37	19 38	3·6	14 40	18 59	23 16	5 7 0	21 28	2·6	0 18	8 22	16 24
Mar. 2	15 47 30	19 44	3·7	12 47	17 3	21 20	5 6 13	21 29	2·6	22 19	6 23	14 26
Apr. 1	15 46 0	19 39	3·7	10 44	15 3	19 20	5 7 36	21 32	2·6	20 21	4 26	12 29
May 1	15 41 57	19 26	3·8	8 41	13 1	17 19	5 10 51	21 37	2·5	18 22	2 32	10 36
31	15 36 52	19 9	3·8	6 36	10 58	15 18	5 15 47	21 43	2·5	16 32	0 38	8 42
June 30	15 32 38	18 55	3·7	4 33	8 56	13 17	5 20 3	21 48	2·5	14 34	22 41	6 46
July 30	15 30 46	18 49	3·7	2 32	6 56	11 18	5 24 15	21 51	2·5	12 40	20 47	4 52
Aug. 29	15 31 58	18 54	3·6	0 35	4 59	9 22	5 27 3	21 53	2·6	10 45	18 52	2 57
Sept. 28	15 36 9	19 9	3·5	22 44	3 6	7 26	5 27 53	21 52	2·6	8 48	16 55	1 0
Oct. 28	15 42 36	19 31	3·5	20 54	1 14	5 31	5 26 37	21 50	2·6	6 49	14 56	23 1
Nov. 27	15 50 11	19 54	3·4	19 4	23 20	3 36	5 23 40	21 47	2·7	4 48	12 55	21 0
Dec. 27	15 57 30	20 18 S.	3·4	17 14	21 29	1 42	5 20 5	21 44 N.	2·7	2 47	10 53	18 57

Ceres.				Pallas.			
1897.	R.A. Noon.	Dec. Noon.	Transits.	1897.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Sept. 19 ...	6 20	21 55 N.	18 23	Aug. 14 ...	2 41	2 55 S.	17 6
Oct. 13 ...	6 42	22 35	17 10	Sept. 7 ...	2 53	8 20	15 43
Nov. 6 ...	6 52	23 40	15 45	Oct. 1 ...	2 52	15 30	14 8
30 ...	6 46	25 24	14 5	25 ...	2 38	22 25	12 19
Dec. 24 ...	6 25	27 27 N.	12 9	Nov. 18 ...	2 19	26 24	10 26
				Dec. 12 ...	2 8	26 30 S.	8 41

Juno.				Vesta.			
Nov. 6 ...	12 25	2 2 S.	21 19	Nov. 6 ...	12 31	2 12 N.	21 25
30 ...	12 56	4 19	20 16	30 ...	13 13	1 35 S.	20 32
Dec. 24 ...	13 23	5 52	19 8	Dec. 24 ...	13 53	4 47 S.	19 38

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1897.

In the year 1897 there will be two Eclipses, both of the Sun.

I.—*An Annular Eclipse of the Sun*, 1897, *February* 1.

Invisible at Greenwich. The path of the annulus during the Eclipse lies almost wholly in the South Pacific. Starting from a point to the north-west of New Zealand, and just touching the north point of the island, it crosses the Ocean to New Granada and Venezuela, and at Trinidad the setting Sun is seen annularly eclipsed. The Eclipse will be seen as partial at South American and Mexican observatories.

Greenwich M.T. of ☌ in R.A.: Feb. 1^{d} 8^{h} 6^{m} 41^{s}·6.

		d	h	m		°	′		°	′	
Eclipse begins ...	Feb.	1	5	23·0	G.M.T. in Long.	176	31	W., Lat.	28	6	S.
Eclipse ends	Feb.	1	11	7·8		77	59	W.,	14	42	N.

II.—*An Annular Eclipse of the Sun*, 1897, *July* 29.

Invisible at Greenwich. The line of central Eclipse on this occasion also lies almost entirely in the Ocean. Beginning in the Pacific, it crosses Mexico and Cuba, and continues its course in the South Atlantic. The Eclipse will be seen as partial over almost the whole of North and South America and from the west coast of Africa. The Sun will be seen eclipsed annularly from Antigua.

Greenwich M.T. of ☌ in R.A.: July 29^{d} 3^{h} 59^{m} 50^{s}·5.

		d	h	m		°	′		°	′	
Eclipse begins......	July	29	1	2·3	in Long.	109	52	W., Lat.	17	0	N.
Eclipse ends	July	29	6	52·1		19	3	W.,	21	32	S.

There will be a Total Eclipse of the Sun on 1898, *Jan.* 21.

Invisible at Greenwich. The centre of the shadow after crossing Central Africa and the Indian Ocean reaches the Continent of Asia at a point south of Bombay, near Rajapur, where the duration of totality is about 122 seconds. Leaving this in a north-easterly direction, the central line crosses Mt. Everest, goes through China, and ends in Russian Asia. Towns near the line of central Eclipse are Nagpur and Benares, where the duration of totality is 78 and 104 seconds respectively. The Eclipse will happen at these places about one hour after noon on January 22 (local).

Greenwich M.T. of ☌ in R.A.: Jan. 21^{d} 19^{h} 37^{m} 27^{s}·2.

⊙'s and ☾'s R.A. 20^{h} 18^{m} 32^{s}·86. Hourly motion 10^{s}·52 and 147^{s}·73.

	°	′	″		′	″
⊙'s Declination	19	38	39·9 S.	Hourly motion	0	34·5 N.
☽'s Declination	19	6	26·9 S.	Hourly motion	11	34·0 N.
⊙'s Equat. Hor. Parallax			8·99	True Semidiameter...	16	14·83
☽'s Equat. Hor. Parallax		60	11·65	True Semidiameter ...	16	24·30

		d	h	m		°	′		°	′	
Central Eclipse begins	Jan.	21	17	48·9	G.M.T. in Long.	9	46	E., Lat.	11	14	N.
Central Eclipse ends	Jan.	21	20	49·8	G.M.T. in Long.	119	9	E., Lat.	45	47	N.

Date.	Star.	Mag.	Disap.		Reap.	
			M.T.	P.	M.T.	P.
Jan.			h m	°	h m	°
6	*e'* Aquarii	6·5	5 14	128	5 35	163
7	B.A.C. 7986 ...	5·8	4 16	39	5 30	247
7	B.A.C. 7993 ...	6·5	5 38	81	6 42	209
13	19 Tauri.........	4·4	13 30	107	14 26	242
13	20 Tauri.........	4·0	13 58	142	14 30	209
16	ε Geminorum...	3·2	19 6	161	19 30†	222
21	p^3 Leonis	5·7	17 53	96	18 50	330
24	75 Virginis......	5·7	12 44	58	13 12	3
24	D.M. − 15° 3715	6·5	16 46	68	17 30	354
29	φ Sagittarii......	3·3	17 32†	68	18 34*	288
Feb.						
8	26 Arietis	6·1	9 53	99	10 53	233
10	χ Tauri (1st *).	5·7	15 3	138	15 35†	218
13	37 Geminorum.	6·2	7 37	115	8 54	265
13	48 Geminorum.	5·8	16 32	129	17 19	263
15	δ Cancri.........	4·3	7 14	194	7 20	203
15	B.A.C. 3029 ...	6·5	12 54	112	14 1	307
16	18 Leonis	6·1	11 56	169	12 46	257
17	35 Sext. (2nd *)	6·1	14 57	159	15 51	270
18	B.A.C. 3873 ...	6·2	7 45	140	8 38	277
19	B.A.C. 4201 ...	6·3	15 17	150	16 20	279
19	*q* Virginis	5·7	18 33	129	19 32	285
24	43 Ophiuchi ...	5·6	15 40*	45	16 23	331
Mar.						
9	19 Tauri.........	4·4	4 53	116	5 57	216
9	18 Tauri.........	6·3	5 14	21	6 2	310
12	ε Geminorum...	3·2	13 49	167	14 14	221
17	p^3 Leonis	5·7	11 26	138	12 34	294
19	D.M. − 10° 3570	6·0	11 39	152	12 39	277
22	4 Scorpii.........	5·5	12 14*	123	13 17	276
25	B.A.C. 6562 ...	6·3	16 37	91	17 52	247
Apr.						
8	**Mars**............	...	20 12†	104	21 2*	260
9	48 Geminorum.	5·8	9 54	103	10 56	295
11	B.A.C. 3029 ...	6·5	8 8	71	8 56	349
12	18 Leonis	6·1	7 50	125	9 4	301
12	R Leonis	var.	8 59	162	9 56	265
13	35 Sext. (2nd *)	6·1	11 39	123	12 44	303
15	B.A.C. 4201 ...	6·3	11 33	157	12 31	271
15	*q* Virginis	5·7	14 42	136	15 40	278
16	D.M. − 13° 3692	6·5	9 53	91	10 51	335
20	X Sagittarii ...	var.	14 52	49	15 49	309
21	σ Sagittarii ...	2·3	16 48	64	18 3	268
May						
4	D.M. + 26° 783	6·5	7 48	63	8 38	307
6	ω Geminorum .	5·3	11 51	78	12 36†	309
9	B.A.C. 3270 ...	6·5	11 47	63	12 20	350
11	p^3 Leonis	5·7	7 14	80	8 4	352
13	D.M. − 10° 3570	6·0	9 10	111	10 18	316
16	π Scorpii	3·1	9 15	100	10 20	295
17	B.A.C. 5792 ...	6·3	11 52	65	12 55	305
18	B.A.C. 6194 ...	4·7	11 33	13	11 48	346
23	B.A.C. 7986 ...	5·8	14 22	88	15 23	212

Date.	Star.	Mag.	Disap.		Rea
			M.T.	P.	M.T.
June			h m	°	h m
4	θ Cancri.........	5·8	11 11‡	142	11 53
9	*q* Virginis	5·7	9 11	86	10 7
14	X Sagittarii ...	var.	11 14	96	12 27
15	σ Sagittarii ...	2·3	12 0	129	12 47
17	19 Capricorni...	6·0	12 21	24	13 14
17	21 Capricorni...	6·3	15 39	93	16 40
18	B.A.C. 7599 ...	6·5	13 42	128	14 10
July					
11	B.A.C. 5792 ...	6·3	8 3	66	9 7
12	B.A.C. 6194 ...	4·7	7 53	40	8 38
13	χ' Sagittarii ...	5·1	9 21	46	10 21
16	B.A.C. 7774 ...	6·2	12 50	35	13 59
18	22 Piscium......	5·9	13 46	38	14 57
23	17 Tauri.........	3·8	12 17	57	13 10
23	16 Tauri.........	6·5	12 42	355	12 53
23	23 Tauri.........	4·2	12 56	121	13 33
23	η Tauri	3·0	13 23	99	14 15
23	28 Tauri.........	6·2	14 17	126	14 53
Aug.					
4	89 Virginis......	5·2	8 17	148	9 11
9	σ Sagittarii......	2·3	6 10	155	6 56
11	19 Capricorni...	6·0	7 16	40	8 14
11	21 Capricorni...	6·3	10 29	86	11 37
20	χ Tauri (1st *).	5·7	13 41	55	14 44
23	ω Geminorum..	5·3	13 10	73	14 1
Sept.					
9	B.A.C. 7774 ...	6·2	5 55	9	6 30
14	26 Arietis	6·1	18 27	63	19 36
15	ε Arietis.........	4·6	6 32†	66	7 22
18	125 Tauri	4·9	8 47†	95	9 36
18	139 Tauri	5·1	17 57	136	19 2
Oct.					
3	χ' Sagittarii ...	5·1	6 43	118	7 34
8	λ Piscium	4·7	9 31	21	10 35
8	22 Piscium......	5·9	16 2	123	16 32
12	μ Arietis.........	5·8	5 29	76	6 21
13	17 Tauri.........	3·8	11 59	39	13 6
13	23 Tauri.........	4·2	12 41	103	13 51
13	η Tauri	3·0	13 25	86	14 47
13	28 Tauri.........	6·2	14 34	104	15 49
13	27 Tauri.........	3·8	14 41	128	15 35
15	125 Tauri	4·9	17 47	132	18 51
16	B.A.C. 2058 ...	6·5	12 51	146	13 33
17	58 Geminorum.	6·1	16 20	98	17 43
Nov.					
17	43 Leonis	6·5	15 43	106	16 54
18	p^5 Leonis	5·5	18 4	166	19 0
20	D.M. − 10° 3570	6·0	16 45	93	17 40
30	D.M. − 7° 5797	6·2	9 32	87	10 27
Dec.					
2	22 Piscium......	5·9	1 25	77	2 24
5	26 Arietis	6·1	14 50	27	15 29
6	ε Arietis.........	4·6	2 30	35	3 14
6	66 Arietis	6·1	18 28‡	122	19 8
12	*d'* Cancri	5·9	9 26	131	10 24
17	B.A.C. 4201 ...	6·3	13 25	67	14 1

* Star rising. ‡ Star setting. † Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	Aug.			h m	°
13	18 Tauri ……	6·3	14 0	355	6	*b* Scorpii ……	4·8	10 12	6
17	D.M.+23°1780	6·1	19 34	17	10	B.A.C. 6850 …	6·5	8 32	3
18	μ^2 Cancri ……	5·3	5 11	10	Sept.				
20	A Leonis ……	4·6	12 32	33	6	53 Sagittarii …	6·2	7 27	344
Feb.					6	B.A.C. 6727 …	6·2	7 38	343
20	D.M.−13°3692	6·5	15 23	32	11	22 Piscium……	5·9	7 8	332
Mar.					19	ε Geminor……	3·2	13 54	1
13	D.M.+23°1780	6·1	14 41	19	Oct.				
16	A Leonis ……	4·6	7 14	29	3	χ^3 Sagittarii …	5·9	7 40	340
22	π Scorpii ……	3·1	14 11	17	13	16 Tauri ……	6·5	12 35	341
May					30	B.A.C. 6490 …	5·7	4 29	346
16	B.A.C. 5347 …	5·8	14 10	8	Nov.				
19	χ' Sagittarii …	5·1	13 32	348	28	υ Capricorni …	5·3	5 20	332
June					30	ρ Aquarii ……	5·4	2 54	331
16	B.A.C. 6850 …	6·5	13 52	340	Dec.				
July					7	27 Tauri………	3·8	2 47	346
10	B.A.C. 5347 …	5·8	9 24	10	17	*q* Virginis ……	5·7	17 4	34
23	20 Tauri ……	4·0	13 15	343					

South Near Approaches.

Jan.			h m	°	Aug.			h m	°
13	16 Tauri …..	6·5	13 45	175	12	B.A.C. 7599…	6·5	8 40	154
14	χ Tauri (1st *).	5·7	4 38	163	Oct.				
Feb.					12	ε Arietis ……	4·6	15 47	165
16	R Leonis ……	Var	13 12	213	19	o^2 Cancri ……	5·6	13 1	196
Mar.					Nov.				
9	20 Tauri ……	4·0	5 47	167	9	66 Arietis ……	6·1	11 21	161
May					13	B.A.C. 2238 …	6·5	8 16	182
16	4 Scorpii ……	5·5	8 32	199	Dec.				
17	43 Ophiuchi …	5·6	17 21	174	10	B.A.C. 2238 …	6·5	16 20	198
July					14	B.A.C. 3398 …	6·0	9 10	200
11	43 Ophiuchi …	5·6	13 42	173	18	69 Virginis …	4·9	18 23	211
22	ε Arietis………	4·6	14 47	156	26	29 Capricorni..	5·5	7 2	151
23	27 Tauri………	3·8	14 33	161					

Apparent Disk of Mars, 1897.

Jan. 1	0·978	May 1	0·912	Aug. 29	0·981
31	0·926	May 31	0·929	Sept. 28	0·992
Mar. 2	0·901	June 30	0·948	Oct. 28	0·999
April 1	0·900	July 30	0·965	Nov. 27	1·000
				Dec. 27	0·997

The numbers in this table are the versed sines of the illuminated disk, the apparent diameter of the planet being taken as unity.

Mars not being in opposition during the year 1897, the satellites will not be visible.

JUPITER'S SATELLITES, 1897.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

January.

d	h	m	
1	16	42	i. *e*.
	17	10	ii. *e*.
	20	2	i. O.
	22	10	ii. O.
2	14	57	i. *t*.
	17	16	i. T.
3	11	10	i. *e*.
	13	51	ii. *t*.
	14	29	i. O.
	16	42	ii. T.
	17	27	iii. *t*.
	20	57	iii. T.
4	9	24	i. *t*.
	11	44	i. T.
5	5	38	i. *e*.
	6	29	ii. *e*.
	8	56	i. O.
	11	22	ii. O.
6	3	51	i. *t*.
	6	11	i. T.
	8	10	iv. *e*.
	12	41	iv. E.
	17	42	iv. *o*.
	21	57	iv. O.
7	0	7	i. *e*.
	3	2	ii. *t*.
	3	19	iii. *e*.
	3	22	i. O.
	5	52	ii. T.
	6	47	iii. E.
	7	15	iii. *o*.
	10	44	iii. O.
	22	18	i. *t*.
8	0	37	i. T.
	18	35	i. *e*.
	19	46	ii. *e*.
	21	49	i. O.
9	0	34	ii. O.
	16	45	i. *t*.
	19	4	i. T.
10	13	3	i. *e*.
	16	12	ii. *t*.
	16	16	i. O.
	19	2	ii. T.
	21	1	iii. *t*.
11	0	30	iii. T.
	11	11	i. *t*.
	13	31	i. T.
12	7	31	i. *e*.
	9	5	ii. *e*.
	10	42	i. O.
	13	45	ii. O.
13	5	38	i. *t*.
	7	58	i. T.

Jan. (con.).

d	h	m	
14	2	0	i. *e*.
	5	9	i. O.
	5	21	ii. *t*.
	7	16	iii. *e*.
	8	12	ii. T.
	10	43	iii. E.
	10	46	iii. *o*.
	14	14	iii. O.
	23	59	iv. *t*.
15	0	5	i. *t*.
	2	24	i. T.
	4	13	iv. T.
	20	28	i. *e*.
	22	22	ii. *e*.
	23	35	i. O.
16	2	56	ii. O.
	18	32	i. *t*.
	20	51	i. T.
17	14	56	i. *e*.
	18	2	i. O.
	18	31	ii. *t*.
	21	21	ii. T.
18	0	30	iii. *t*.
	3	59	iii. T.
	12	58	i. *t*.
	15	18	i. T.
19	9	25	i. *e*.
	11	41	ii. *e*.
	12	28	i. O.
	16	6	ii. O.
20	7	25	i. *t*.
	9	44	i. T.
21	3	53	i. *e*.
	6	54	i. O.
	7	39	ii. *t*.
	10	29	ii. T.
	11	13	iii. *e*.
	17	40	iii. O.
22	1	51	i. *t*.
	4	10	i. T.
	22	21	i. *e*.
23	0	59	ii. *e*.
	1	21	i. O.
	2	9	iv. *e*.
	5	16	ii. O.
	6	38	iv. E.
	9	0	iv. *o*.
	13	12	iv. O.
	20	18	i. *t*.
	22	36	i. T.
24	16	50	i. *e*.
	19	47	i. O.
	20	47	ii. *t*.
	23	38	ii. T.

Jan. (con.).

d	h	m	
25	3	56	iii. *t*.
	7	24	iii. T.
	14	44	i. *t*.
	17	3	i. T.
26	11	18	i. *e*.
	14	13	i. O.
	14	18	ii. *e*.
	18	25	ii. O.
27	9	10	i. *t*.
	11	29	i. T.
28	5	46	i. *e*.
	8	39	i. O.
	9	55	ii. *t*.
	12	45	ii. T.
	15	11	iii. *e*.
	21	3	iii. O.
29	3	37	i. *t*.
	5	56	i. T.
30	0	15	i. *e*.
	3	5	i. O.
	3	35	ii. *e*.
	7	34	ii. O.
	22	3	i. *t*.
31	0	22	i. T.
	14	51	iv. *t*.
	18	43	i. *e*.
	19	4	iv. T.
	21	31	i. O.
	23	2	ii. *t*.

February.

d	h	m	
1	1	52	ii. T.
	7	17	iii. *t*.
	10	45	iii. T.
	16	29	i. *t*.
	18	48	i. T.
2	13	11	i. *e*.
	15	57	i. O.
	16	54	ii. *e*.
	20	42	ii. O.
3	10	55	i. *t*.
	13	14	i. T.
4	7	40	i. *e*.
	10	23	i. O.
	12	9	ii. *t*.
	14	59	ii. T.
	19	10	iii. *e*.
5	0	23	iii. O.
	5	21	i. *t*.
	7	40	i. T.
6	2	8	i. *e*.
	4	49	i. O.
	6	12	ii. *e*.

Feb. (con.).

d	h	m	
6	9	51	ii. O.
	23	47	i. *t*.
7	2	6	i. T.
	20	36	i. *e*.
	23	15	i. O.
8	1	16	ii. *t*.
	4	6	ii. T.
	10	35	iii. *t*.
	14	4	iii. T.
	18	13	i. *t*.
	20	9	iv. *e*.
	20	32	i. T.
9	3	43	iv. O.
	15	5	i. *e*.
	17	41	i. O.
	19	30	ii. *e*.
	22	59	ii. O.
10	12	39	i. *t*.
	14	58	i. T.
11	9	33	i. *e*.
	12	7	i. O.
	14	23	ii. *t*.
	17	13	ii. T.
	23	8	iii. *e*.
12	3	41	iii. O.
	7	5	i. *t*.
	9	24	i. T.
13	4	2	i. *e*.
	6	33	i. O.
	8	48	ii. *e*.
	12	6	ii. O.
14	1	31	i. *t*.
	3	50	i. T.
	22	30	i. *e*.
15	0	59	i. O.
	3	29	ii. *t*.
	6	20	ii. T.
	13	52	iii. *t*.
	17	21	iii. T.
	19	57	i. *t*.
	22	16	i. T.
16	16	59	i. *e*.
	19	25	i. O.
	22	7	ii. *e*.
17	1	14	ii. O.
	5	8	iv. *t*.
	9	23	iv. T.
	14	23	i. *t*.
	16	42	i. T.
18	11	27	i. *e*.
	13	50	i. O.
	16	36	ii. *t*.
	19	26	ii. T.
19	3	6	iii. *e*.

Feb. (con.).

d	h	m	
19	6	57	iii. O.
	8	49	i. *t*.
	11	8	i. T.
20	5	55	i. *e*.
	8	16	i. O.
	11	24	ii. *e*.
	14	22	ii. O.
21	3	15	i. *t*.
	5	34	i. T.
22	0	24	i. *e*.
	2	42	i. O.
	5	42	ii. *t*.
	8	33	ii. T.
	17	7	iii. *t*.
	20	37	iii. T.
	21	41	i. *t*.
23	0	0	i. T.
	18	49	i. *o*.
	21	8	i. O.
24	0	39	ii. *o*.
	3	31	ii. E.
	16	7	i. *t*.
	18	26	i. T.
25	13	15	i. *o*.
	13	36	iv. *o*.
	15	35	i. E.
	18	31	iv. E.
	18	49	ii. *t*.
	21	39	ii. T.
26	6	43	iii. *o*.
	10	30	iii. E.
	10	33	i. *t*.
	12	52	i. T.
27	7	41	i. *o*.
	10	4	i. E.
	13	46	ii. *o*.
	16	49	ii. E.
28	4	59	i. *t*.
	7	18	i. T.

March.

d	h	m	
1	2	7	i. *o*.
	4	32	i. E.
	7	55	ii. *t*.
	10	46	ii. T.
	20	23	iii. *t*.
	23	25	i. *t*.
	23	54	iii. T.
2	1	44	i. T.
	20	32	i. *o*.
	23	1	i. E.
3	2	54	ii. *o*.
	6	7	ii. E.

Mar. (con.).

d	h	m	
3	17	51	i. *t*.
	20	10	i. T
4	14	58	i. *o*.
	17	29	i. E
	21	2	ii. *t*
	23	53	ii.
5	9	59	iii.
	12	16	i. *t*.
	14	28	iii.
	14	35	i. T
	19	14	iv.
	23	33	iv.
6	9	24	i. *o*.
	11	58	i. E
	16	1	ii. *o*
	19	25	ii.
7	6	43	i. *t*.
	9	2	i. T
8	3	50	i. *o*.
	6	26	i. E
	10	9	ii. *t*
	13	0	ii. T
	23	40	iii.
9	1	9	i. *t*.
	3	11	iii.
	3	28	i. T
	22	16	i. *o*.
10	0	55	i. E
	5	9	ii. *o*
	8	43	ii. E
	19	35	i. *t*.
	21	54	i. T
11	16	43	i. *o*.
	19	23	i. E
	23	16	ii. *t*
12	2	7	ii.
	13	16	iii.
	14	1	i. *t*.
	16	21	i. T
	18	25	iii.
13	11	9	i. *o*.
	13	52	i. E
	18	17	ii. *o*
	22	0	ii.
14	3	46	iv.
	8	8	iv.
	8	11	iv.
	8	28	i. *t*.
	10	47	i. T
	12	28	iv.
15	5	35	i. *o*.
	8	21	i. E
	12	24	ii. *t*
	15	16	ii.

Mar. (con.).

d	h	m	
16	2	54	i. *t*.
	2	59	iii. *t*.
	5	13	i. T.
	6	31	iii. T.
17	0	1	i. *o*.
	2	49	i. E.
	7	26	ii. *o*.
	11	19	ii. E.
	21	20	i. *t*.
	23	39	i. T.
18	18	27	i. *o*.
	21	18	i. E.
19	1	32	ii. *t*.
	4	23	ii. T.
	15	47	i. *t*.
	16	35	iii. *o*.
	18	6	i. T.
	22	24	iii. E.
20	12	54	i. *o*.
	15	46	i. E.
	20	35	ii. *o*.
21	0	37	ii. E.
	10	13	i. *t*.
	12	32	i. T.
22	7	20	i. *o*.
	9	39	iv. *t*.
	10	15	i. E.
	14	4	iv. T.
	14	41	ii. *t*.
	17	32	ii. T.
23	4	39	i. *t*.
	6	20	iii. *t*.
	6	59	i. T.
	9	52	iii. T.
24	1	47	i. *o*.
	4	44	i. E.
	9	44	ii. *o*.
	13	55	ii. E.
	23	6	i. *t*.
25	1	25	i. T.
	20	13	i. *o*.
	23	12	i. E.
26	3	49	ii. *t*.
	6	41	ii. T.
	17	32	i. *t*.
	19	52	i. T.
	19	58	iii. *o*.
27	2	23	iii. E.
	14	39	i. *o*.
	17	41	i. E.
	22	53	ii. *o*.
28	3	13	ii. E.
	11	59	i. *t*.
	14	18	i. T.
29	9	6	i. *o*.
	12	9	i. E.
	16	69	ii. *t*.
	19	51	ii. T.
30	6	26	i. *t*.
	8	45	i. T.
	9	44	iii. *t*.
	13	18	iii. T.
	18	28	iv. *o*.
	22	56	iv. O.

Mar. (con.).

d	h	m	
31	2	13	iv. *e*.
	3	33	i. *o*.
	6	26	iv. E.
	6	38	i. E.
	12	4	ii. *o*.
	16	31	ii. E.

April.

d	h	m	
1	0	53	i. *t*.
	3	12	i. T.
	22	0	i. *o*.
2	1	7	i. E.
	6	9	ii. *t*.
	9	1	ii. T.
	19	19	i. *t*.
	21	38	i. T.
	23	26	iii. *o*.
3	2	59	iii. O.
	2	59	iii. *e*.
	6	21	iii. E.
	16	26	i. *o*.
	19	35	i. E.
4	1	14	ii. *o*.
	5	49	ii. E.
	13	46	i. *t*.
	16	5	i. T.
5	10	53	i. *o*.
	14	4	i. E.
	19	20	ii. *t*.
	22	12	ii. T.
6	8	13	i. *t*.
	10	32	i. T.
	13	13	iii. *t*.
	16	47	iii. T.
7	5	20	i. *o*.
	8	33	i. E.
	14	25	ii. *o*.
	19	7	ii. E.
8	0	44	iv. *t*.
	2	40	i. *t*.
	5	0	i. T.
	5	14	iv. T.
	23	47	i. *o*.
9	3	1	i. E.
	8	31	ii. *t*.
	11	23	ii. T.
	21	8	i. *t*.
	23	27	i. T.
10	2	57	iii. *o*.
	6	31	iii. O.
	6	59	iii. *e*.
	10	20	iii. E.
	18	15	i. *o*.
	21	30	i. E.
11	3	37	ii. *o*.
	8	25	ii. E.
	15	35	i. *t*.
	17	54	i. T.
12	12	42	i. *o*.
	15	59	i. E.
	21	43	ii. *t*.
13	0	35	ii. T.
	10	2	i. *t*.

Apr. (con.).

d	h	m	
13	12	21	i. T.
	16	46	iii. *t*.
	20	21	iii. T.
14	7	9	i. *o*.
	10	28	i. E.
	16	49	ii. *o*.
	21	43	ii. E.
15	4	29	i. *t*.
	6	48	i. T.
16	1	36	i. *o*.
	4	56	i. E.
	9	59	iv. *o*.
	10	55	ii. *t*.
	13	48	ii. T.
	14	31	iv. O.
	20	15	iv. *e*.
	22	56	i. *t*.
17	0	24	iv. E.
	1	16	i. T.
	6	32	iii. *o*.
	10	7	iii. O.
	10	58	iii. *e*.
	14	19	iii. E.
	20	4	i. *o*.
	23	25	i. E.
18	6	2	ii. *o*.
	11	0	ii. E.
	17	24	i. *t*.
	19	43	i. T.
19	14	31	i. *o*.
	17	54	i. E.
20	0	8	ii. *t*.
	3	1	ii. T.
	11	51	i. *t*.
	14	11	i. T.
	20	25	iii. *t*.
21	0	0	iii. T.
	8	59	i. *o*.
	12	22	i. E.
	19	15	ii. *o*.
22	0	18	ii. E.
	6	19	i. *t*.
	8	38	i. T.
23	3	26	i. *o*.
	6	51	i. E.
	13	22	ii. *t*.
	16	15	ii. T.
24	0	47	i. *t*.
	3	6	i. T.
	10	12	iii. *o*.
	13	46	iii. O.
	14	57	iii. *e*.
	16	43	iv. *t*.
	18	17	iii. E.
	21	16	iv. T.
	21	54	i. *o*.
25	1	20	i. E.
	8	29	ii. *o*.
	13	36	ii. E.
	19	14	i. *t*.
	21	34	i. T.
26	16	22	i. *o*.
	19	49	i. E.
27	2	36	ii. *t*.

Apr. (con.).

d	h	m	
27	5	29	ii. T.
	13	42	i. *t*.
	16	2	i. T.
28	0	7	iii. *t*.
	3	42	iii. T.
	10	50	i. *o*.
	14	17	i. E.
	21	43	ii. *o*.
29	2	54	ii. E.
	8	10	i. *t*.
	10	30	i. T.
30	5	18	i. *o*.
	8	46	i. E.
	15	51	ii. *t*.
	18	44	ii. T.

May.

d	h	m	
1	2	38	i. *t*.
	4	57	i. T.
	13	57	iii. *o*.
	17	32	iii. O.
	18	56	iii. *e*.
	22	16	iii. E.
	23	46	i. *o*.
2	3	15	i. E.
	10	58	ii. *o*.
	16	11	ii. E.
	21	6	i. *t*.
	23	25	i. T.
3	2	28	iv. *o*.
	7	4	iv. O.
	14	19	iv. *e*.
	18	14	i. *o*.
	18	22	iv. E.
	21	44	i. E.
4	5	6	ii. *t*.
	7	59	ii. T.
	15	34	i. *t*.
	17	53	i. T.
5	3	54	iii: *t*.
	7	30	iii. T.
	12	42	i. *o*.
	16	12	i. E.
6	0	13	ii. *o*.
	5	29	ii. E.
	10	2	i. *t*.
	12	22	i. T.
7	7	10	i. *o*.
	10	41	i. E.
	18	22	ii. *t*.
	21	15	ii. T.
8	4	30	i. *t*.
	6	50	i. T.
	17	46	iii. *o*.
	21	22	iii. O.
	22	56	iii. *e*.
9	1	38	i. *o*.
	2	15	iii. E.
	5	10	i. E.
	13	29	ii. *o*.
	18	47	ii. E.
	22	59	i. *t*.
10	1	18	i. T.

May (con.).

d	h	m	
10	20	7	i. *o*.
	23	39	i. E.
11	7	39	ii. *t*.
	9	40	iv. *t*.
	10	32	ii. T.
	14	15	iv. T.
	17	27	i. *t*.
	19	46	i. T.
12	7	45	iii. *t*.
	11	21	iii. T.
	14	35	i. *o*.
	18	7	i. E.
13	2	46	ii. *o*.
	8	5	ii. E.
	11	55	i. *t*.
	14	15	i. T.
14	9	4	i. *o*.
	12	36	i. E.
	20	56	ii. *t*.
	23	49	ii. T.
15	6	24	i. *t*.
	8	43	i. T.
	21	40	iii. *o*.
16	1	16	iii. O.
	2	56	iii. *e*.
	3	32	i. *o*.
	6	14	iii. E.
	7	5	i. E.
	16	2	ii. *o*.
	21	22	ii. E.
17	0	52	i. *t*.
	3	12	i. T.
	22	1	i. *o*.
18	1	34	i. E.
	10	14	ii. *t*.
	13	7	ii. T.
	19	21	i. *t*.
	21	40	i. T.
19	11	40	iii. *t*.
	15	16	iii. T.
	16	30	i. *o*.
	19	55	iv. *o*.
	20	3	i. E.
20	0	32	iv. O.
	5	20	ii. *o*.
	8	22	iv. *e*.
	10	40	ii. E.
	12	19	iv. E.
	13	49	i. *t*.
	16	9	i. T.
21	10	58	i. *o*.
	14	31	i. E.
	23	32	ii. *t*.
22	2	25	ii. T.
	8	18	i. *t*.
	10	38	i. T.
23	1	38	iii. *o*.
	5	14	iii. O.
	5	27	i. *o*.
	6	56	iii. *e*.
	9	0	i. E.
	10	13	iii. E.
	18	38	ii. *o*.
	23	57	ii. E.

May (con.

d	h	m	
24	2	47	i.
	5	6	i.
	23	56	i.
25	3	29	i.
	12	51	ii.
	15	44	ii.
	21	16	i.
	23	35	i.
26	15	40	iii
	18	25	i.
	19	15	iii.
	21	58	i.
27	7	56	ii.
	13	15	ii.
	15	45	i.
	18	4	i.
28	3	31	iv.
	8	7	iv.
	12	54	i.
	16	26	i.
29	2	10	ii.
	5	3	ii.
	10	13	i.
	12	33	i.
30	5	40	iii.
	7	23	i.
	9	15	iii.
	10	55	i.
	10	56	iii.
	14	13	iii
	21	14	ii.
31	2	32	ii.
	4	42	i.
	7	2	i.

June.

d	h	m	
1	1	52	i.
	5	24	i.
	15	30	ii.
	18	23	ii.
	23	11	i.
2	1	31	i.
	19	43	iii
	20	21	i.
	23	18	iii
	23	53	i.
3	10	33	ii.
	15	49	ii.
	17	40	i.
	20	0	i.
4	14	50	i.
	18	22	i.
5	4	50	ii.
	7	42	ii.
	12	9	i.
	14	13	iv.
	14	29	i.
	18	49	iv.
6	2	25	iv.
	6	17	iv.
	9	19	i.
	9	45	iii
	12	50	i.

June (con.).

d	h	m	
6	13	20	iii. O.
	14	55	iii. *e.*
	18	11	iii. E.
	23	52	ii. *o.*
7	5	7	ii. E.
	6	38	i. *t.*
	8	58	i. T.
8	3	49	i. *o.*
	7	19	i. E.
	18	11	ii. *t.*
	21	4	ii. T.
9	1	8	i. *t.*
	3	27	i. T.
	22	18	i. *o.*
	23	50	iii. *t.*
10	1	48	i. E.
	3	25	iii. T.
	13	12	ii. *o.*
	18	24	ii. E.
	19	37	i. *t.*
	21	56	i. T.
11	16	47	i. *o.*
	20	17	i. E.
12	7	31	ii. *t.*
	10	24	ii. T.
	14	6	i. *t.*
	16	26	i. T.
13	11	17	i. *o.*
	13	53	iii. *o.*
	14	46	i. E.
	17	28	iii. O.
	18	55	iii. *e.*
	22	9	iv. *t.*
	22	10	iii. E.
14	2	32	ii. *o.*
	2	44	iv. T.
	7	41	ii. E.
	8	36	i. *t.*
	10	55	i. T.
15	5	46	i. *o.*
	9	14	i. E.
	20	53	ii. *t.*
	23	46	ii. T.
16	3	5	i. *t.*
	5	24	i. T.
17	0	16	i. *o.*
	3	43	i. E.
	3	59	iii. *t.*
	7	36	iii. T.
	15	52	ii. *o.*
	20	59	ii. E.
	21	34	i. *t.*
	23	54	i. T.
18	18	45	i. *o.*
	22	12	i. E.
19	10	14	ii. *t.*
	13	7	ii. T.
	16	4	i. *t.*
	18	23	i. T.
20	13	15	i. *o.*
	16	41	i. E.
	18	5	iii. *o.*
	21	39	iii. O.
	22	54	iii. *e.*

June (con.).

d	h	m	
21	2	9	iii. E.
	5	13	ii. *o.*
	10	16	ii. E.
	10	34	i. *t.*
	12	53	i. T.
22	7	45	i. *o.*
	9	15	iv. *o.*
	11	10	i. E.
	13	48	iv. O.
	20	28	iv. *e.*
	23	37	ii. *t.*
23	0	14	iv. E.
	2	29	ii. T.
	5	3	i. *t.*
	7	22	i. T.
24	2	14	i. *o.*
	5	38	i. E.
	8	12	iii. *t.*
	11	47	iii. T
	18	34	ii. *o.*
	23	33	i. *t.*
	23	33	ii. E.
25	1	52	i. T.
	20	44	i. *o.*
26	0	7	i. E.
	12	59	ii. *t.*
	15	51	ii. T.
	18	2	i. *t.*
	20	22	i. T.
27	15	14	i. *o.*
	18	36	i. E.
	22	19	iii. *o.*
28	1	53	iii. O.
	2	54	iii. *e.*
	6	7	iii. E.
	7	55	ii. *o.*
	12	32	i. *t.*
	12	50	ii. E.
	14	51	i. T.
29	9	44	i. *o.*
	13	5	i. E.
30	2	22	ii. *t.*
	5	14	ii. T.
	7	2	i. *t.*
	9	21	i. T.
	17	27	iv. *t.*
	21	59	iv. T.

July.

1	4	14	i. *o.*
	7	33	i. E.
	12	27	iii. *t.*
	16	1	iii. T.
	21	16	ii. *o.*
2	1	31	i. *t.*
	2	7	ii. E.
	3	51	i. T.
	22	44	i. *o.*
3	2	2	i. E.
	15	44	ii. *t.*
	18	36	ii. T.
	20	1	i. *t.*
	22	21	i. T.

July (con.).

d	h	m	
4	17	13	i. *o.*
	20	31	i. E.
5	2	37	iii. *o.*
	6	10	iii. O.
	6	54	iii. *e.*
	10	7	iii. E.
	10	38	ii. *o.*
	14	31	i. *t.*
	15	25	ii. E.
	16	50	i. T.
6	11	43	i. *o.*
	15	0	i. E.
7	5	8	ii. *t.*
	8	0	ii. T.
	9	1	i. *t.*
	11	20	i. T.
8	6	13	i. *o.*
	9	29	i. E.
	16	45	iii. *t.*
	20	18	iii. T.
9	0	0	ii. *o.*
	3	30	i. *t.*
	4	42	ii. E.
	4	51	iv. *o.*
	5	50	i. T.
	9	21	iv. O.
	14	31	iv. *e.*
	18	10	iv. E.
10	0	43	i. *o.*
	3	57	i. E.
	18	31	ii. *t.*
	21	23	ii. T.
	22	0	i. *t.*
11	0	19	i. T.
	19	13	i. *o.*
	22	26	i. E.
12	6	56	iii. *o.*
	10	29	iii. O.
	10	53	iii. *e.*
	13	23	ii. *o.*
	14	6	iii. E.
	16	30	i. *t.*
	17	59	ii. E.
	18	49	i. T.
13	13	43	i. *o.*
	16	55	i. E.
14	7	55	ii. *t.*
	10	47	ii. T.
	11	0	i. *t.*
	13	19	i. T.
15	8	13	i. *o.*
	11	24	i. E.
	21	4	iii. *t.*
16	0	37	iii. T.
	2	45	ii. *o.*
	5	30	i. *t.*
	7	16	ii. E.
	7	49	i. T.
17	2	43	i. *o.*
	5	52	i. E.
	13	16	iv. *t.*
	17	43	iv. T.
	21	19	ii. *t.*
18	0	0	i. *t.*

July (con.).

d	h	m	
18	0	10	ii. T.
	2	19	i. T.
	21	13	i. *o.*
19	0	21	i. E.
	11	17	iii. *o.*
	14	49	iii. O.
	14	53	iii. *e.*
	16	7	ii. *o.*
	18	4	iii. E.
	18	29	i. *t.*
	20	33	ii. E.
	20	49	i. T.
20	15	43	i. *o.*
	18	50	i. E.
21	10	43	ii. *t.*
	13	0	i. *t.*
	13	35	ii. T.
	15	19	i. T.
22	10	14	i. *o.*
	13	18	i. E.
23	1	26	iii. *t.*
	4	57	iii. T.
	5	30	ii. O.
	7	30	i. *t.*
	9	49	i. T.
	9	50	ii. E.
24	4	44	i. *o.*
	7	47	i. E.
25	0	7	ii. *t.*
	2	0	i. *t.*
	2	58	ii. T.
	4	19	i. T.
	23	14	i. *o.*
26	0	54	iv. *o.*
	2	16	i. E.
	5	18	iv. O.
	8	34	iv. *e.*
	12	6	iv. E.
	15	40	iii. *o.*
	18	53	ii. *o.*
	20	30	i. *t.*
	22	3	iii. E.
	22	49	i. T.
	23	7	ii. E.
27	17	44	i. *o.*
	20	45	i. E.
28	13	32	ii. *t.*
	15	0	i. *t.*
	16	23	ii. T.
	17	19	i. T.
29	12	14	i. *o.*
	15	13	i. E.
30	5	49	iii. *t.*
	8	16	ii. *o.*
	9	20	iii. T.
	9	30	i. *t.*
	11	49	i. T.
	12	24	ii. E.
31	6	45	i. *o.*
	9	42	i. E.

August.

1	2	56	ii. *t.*
	4	0	i. *t.*

Aug. (con.).

d	h	m	
1	5	47	ii. T.
	6	19	i. T.
2	1	15	i. *o.*
	4	11	i. E.
	20	4	iii. *o.*
	21	39	ii. *o.*
	22	30	i. *t.*
3	0	49	i. T.
	1	40	ii. E.
	2	0	iii. E.
	9	28	iv. *t.*
	13	48	iv. T.
	19	45	i. *o.*
	22	39	i. E.
4	16	20	ii. *t.*
	17	0	i. *t.*
	19	10	ii. T.
	19	19	i. T.
5	14	15	i. *o.*
	17	8	i. E.
6	10	14	iii. *t.*
	11	2	ii. *o.*
	11	30	i. *t.*
	13	43	iii. T.
	13	49	i. T.
	14	57	ii. E.
7	8	46	i. *o.*
	11	37	i. E.
8	5	42	ii. *t.*
	6	0	i. *t.*
	8	19	i. T.
	8	32	ii. T.
9	3	16	i. *o.*
	6	6	i. E.
10	0	26	ii. *o.*
	0	29	iii. *o.*
	0	30	i. *t.*
	2	49	i. T.
	4	14	ii. E.
	5	59	iii. E.
	21	46	i. *o.*
11	0	34	i. E.
	19	1	i. *t.*
	19	4	ii. *t.*
	21	18	iv. *o.*
	21	20	i. T.
	21	53	ii. T.
12	1	34	iv. O.
	2	37	iv. *e.*
	6	2	iv. E.
	16	17	i. *o.*
	19	3	i. E.
13	13	31	i. *t.*
	13	49	ii. *o.*
	14	39	iii. *t.*
	15	50	i. T.
	17	31	ii. E.
	18	8	iii. T.
14	10	47	i. *o.*
	13	32	i. E.

October.

12	18	37	i. *t.*
	20	55	i. T.

Oct. (con.).

d	h	m	
13	0	40	ii. *e.*
	4	19	ii. O.
	14	41	iii. *e.*
	15	30	i. *e.*
	18	15	i. O.
	19	48	iii. O
14	13	7	i. *t.*
	15	25	i. T.
	20	43	ii. *t.*
	23	26	ii. T.
15	9	58	i. *e.*
	12	45	i. O.
16	7	37	i. *t.*
	9	55	i. T.
	13	57	ii. *e.*
	17	42	ii. O.
17	4	27	i. *e.*
	6	42	iii. *t.*
	7	15	i. O.
	9	53	iii. T.
18	2	7	i. *t.*
	2	44	iv. *e.*
	4	25	i. T.
	5	34	iv. E.
	8	0	iv. *o.*
	10	7	ii. *t.*
	11	12	iv. O.
	12	50	ii. T.
	22	55	i. *e.*
19	1	45	i. O.
	20	37	i. *t.*
	22	55	i. T.
20	3	14	ii. *e.*
	7	5	ii. O.
	17	24	i. *e.*
	18	39	iii. *e.*
	20	15	i. O.
21	0	12	iii. O.
	15	7	i. *t.*
	17	24	i. T.
	23	32	ii. *t.*
22	2	14	ii. T.
	11	52	i. *e.*
	14	45	i. O.
23	9	37	i. *t.*
	11	54	i. T.
	16	31	ii. *e.*
	20	28	ii. O.
24	6	20	i. *e.*
	9	15	i. O.
	11	6	iii. *t.*
	14	14	iii. T.
25	4	7	i. *t.*
	6	24	i. T.
	12	55	ii. *t.*
	15	37	ii. T.
26	0	49	i. *e.*
	3	45	i. O.
	16	30	iv. *t.*
	19	28	iv. T.
	22	37	i. *t.*
27	0	54	i. T.
	5	48	ii. *e.*
	9	51	ii. O.

Oct. (con.).

d	h	m	
17	19	17	i. *e*.
	22	15	i. O.
	22	37	iii. *e*.
28	4	33	iii. O.
	17	7	i. *t*.
	19	24	i. T.
29	2	20	ii. *t*.
	5	1	ii. T.
	13	46	i. *e*.
	16	45	i. O.
30	11	37	i. *t*.
	13	54	i. T.
	19	5	ii. *e*.
	23	14	ii. O.
31	8	14	i. *e*.
	11	15	i. O.
	15	30	iii. *t*.
	18	35	iii. T.

November.

d	h	m	
1	6	6	i. *t*.
	8	23	i. T.
	15	43	ii. *t*.
	18	24	ii. T.
2	2	43	i. *e*.
	5	44	i. O.
3	0	36	i. *t*.
	2	53	i. T.
	8	22	ii. *e*.
	12	36	ii. O.
	20	45	iv. *e*.
	21	11	i. *e*.
	23	25	iv. E.
4	0	14	i. O.
	2	35	iii. *e*.
	4	26	iv. *o*.
	5	33	iii. E.
	5	49	iii. *o*.
	7	8	iv. O.
	8	53	iii. O.
	19	6	i. *t*.
	21	23	i. T.
5	5	7	ii. *t*.
	7	47	ii. T.
	15	39	i. *e*.
	18	45	i. O.
6	13	36	i. *t*.
	15	52	i. T.
	21	39	ii. *e*.

Nov. (con.).

d	h	m	
7	1	59	ii. O.
	10	8	i. *e*.
	13	13	i. O.
	19	52	iii. *t*.
	22	54	iii. T.
8	8	5	i. *t*.
	10	22	i. T.
	18	29	ii. *t*.
	21	9	ii. T.
9	4	36	i. *e*.
	7	43	i. O.
10	2	35	i. *t*.
	4	52	i. T.
	10	56	ii. *e*.
	15	21	ii. O.
	23	4	i. *e*.
11	2	13	i. O.
	6	33	iii. *e*.
	9	29	iii. E.
	10	10	iii. *o*.
	13	11	iii. O.
	21	5	i. *t*.
	23	21	i. T.
12	7	53	ii. *t*.
	10	32	ii. T.
	12	45	iv. *t*.
	15	11	iv. T.
	17	33	i. *e*.
	20	42	i. O.
13	15	34	i. *t*.
	17	50	i. T.
14	0	13	ii. *e*.
	4	43	ii. O.
	12	1	i. *e*.
	15	12	i. O.
15	0	11	iii. *t*.
	3	11	iii. T.
	10	4	i. *t*.
	12	20	i. T.
	21	15	ii. *t*.
	23	53	ii. T.
16	6	30	i. *e*.
	9	41	i. O.
17	4	33	i. *t*.
	6	50	i. T.
	13	31	ii. *e*.
	18	5	ii. O.
18	0	58	i. *e*.
	4	11	i. O.
	10	31	iii. *e*.

Nov. (con.).

d	h	m	
18	13	26	iii. E.
	14	28	iii. *o*.
	17	26	iii. O.
	23	3	i. *t*.
19	1	19	i. T.
	10	37	ii. *t*.
	13	15	ii. T.
	19	26	i. *e*.
	22	40	i. O.
20	14	46	iv. *e*.
	17	15	iv. E.
	17	32	i. *t*.
	19	49	i. T.
21	0	34	iv. *o*.
	2	39	iv. O.
	2	48	ii. *e*.
	7	26	ii. O.
	13	55	i. *e*.
	17	10	i. O.
22	4	29	iii. *t*.
	7	26	iii. T.
	12	2	i. *t*.
	14	18	i. T.
	23	59	ii. *t*.
23	2	37	ii. T.
	8	23	i. *e*.
	11	39	i. O.
24	6	31	i. *t*.
	8	47	i. T.
	16	5	ii. *e*.
	20	47	ii. O.
25	2	51	i. *e*.
	6	8	i. O.
	14	28	iii. *e*.
	17	23	iii. E.
	18	45	iii. *o*.
	21	40	iii. O.
26	1	1	i. *t*.
	3	17	i. T.
	13	21	ii. *t*.
	15	58	ii. T.
	21	20	i. *e*.
27	0	38	i. O.
	19	30	i. *t*.
	21	46	i. T.
28	5	22	ii. *e*.
	10	8	ii. O.
	15	48	i. *e*.
	19	7	i. O.
29	8	39	iv. *t*.

Nov. (con.).

d	h	m	
29	8	44	iii. *t*.
	10	21	iv. T.
	11	37	iii. T.
	13	59	i. *t*.
	16	15	i. T.
30	2	42	ii. *t*.
	5	18	ii. T.
	10	16	i. *e*.
	13	36	i. O.

December.

d	h	m	
1	8	28	i. *t*.
	10	44	i. T.
	18	40	ii. *e*.
	23	29	ii. O.
2	4	44	i. *e*.
	8	5	i. O.
	18	27	iii. *e*.
	21	20	iii. E.
	22	59	iii. *o*.
3	1	51	iii. O.
	2	58	i. *t*.
	5	13	i. T.
	16	3	ii. *t*.
	18	39	ii. T.
	23	13	i. *e*.
4	2	34	i. O.
	21	27	i. *t*.
	23	42	i. T.
5	7	57	ii. *e*.
	12	49	ii. O.
	17	41	i. *e*.
	21	3	i. O.
6	12	56	iii. *t*.
	15	46	iii. T.
	15	56	i. *t*.
	18	11	i. T.
7	5	22	ii. *t*.
	7	58	ii. T.
	8	47	iv. *e*.
	11	4	iv. E.
	12	9	i. *e*.
	15	32	i. O.
	20	17	iv. *o*.
	21	28	iv. O.
8	10	25	i. *t*.
	12	40	i. T.
	21	14	ii. *e*.
9	2	9	ii. O.

Dec. (con.).

d	h	m	
9	6	38	i. *e*.
	10	1	i. O.
	22	25	iii. *e*.
10	1	17	iii. E.
	3	10	iii. *o*.
	4	54	i. *t*.
	5	58	iii. O.
	7	9	i. T.
	18	42	ii. *t*.
	21	17	ii. T.
11	1	6	i. *e*.
	4	30	i. O.
	23	23	i. *t*.
12	1	38	i. T.
	10	31	ii. *e*.
	15	29	ii. O.
	19	34	i. *e*.
	22	59	i. O.
13	17	5	iii. *t*.
	17	52	i. *t*.
	19	52	iii. T.
	20	7	i. T.
14	8	2	ii. *t*.
	10	37	ii. T.
	14	2	i. *e*.
	17	27	i. O.
15	12	20	i. *t*.
	14	36	i. T.
	23	49	ii. *e*.
16	4	48	ii. O.
	8	31	i. *e*.
	11	56	i. O.
17	2	22	iii. *e*.
	5	14	iii. E.
	6	49	i. *t*.
	7	17	iii. *o*.
	9	4	i. T.
	10	3	iii. O.
	21	21	ii. *t*.
	23	55	ii. T.
18	2	59	i. *e*.
	6	25	i. O.
19	1	18	i. *t*.
	3	33	i. T.
	13	6	ii. *e*.
	18	7	ii. O.
	21	27	i. *e*.
20	0	53	i. O.
	19	47	i. *t*.
	21	11	iii. *t*.

Dec. (con.).

d	h	m	
20	22	2	i. T.
	23	54	iii. T
21	10	40	ii. *t*.
	13	13	ii. T.
	15	55	i. *e*.
	19	22	i. O.
22	14	15	i. *t*.
	16	30	i. T.
23	2	24	ii. *e*.
	7	26	ii. O.
	10	24	i. *e*.
	13	50	i. O.
24	2	49	iv. *e*.
	4	53	iv. E
	6	20	iii. *e*.
	8	44	i. *t*.
	9	10	iii. E
	10	59	i. T.
	11	21	iii. *o*.
	14	3	iii. O
	23	58	ii. *t*.
25	2	31	ii. T.
	4	52	i. *e*.
	8	19	i. O.
26	3	12	i. *t*.
	5	27	i. T.
	15	41	ii. *e*.
	20	44	ii. O.
	23	20	i. *e*.
27	2	47	i. O.
	21	41	i. *t*.
	23	56	i. T.
28	1	13	iii. *t*.
	3	54	iii. T
	13	15	ii. *t*.
	15	48	ii. T.
	17	48	i. *e*.
	21	15	i. O.
29	16	9	i. *t*.
	18	24	i. T.
30	4	59	ii. *e*.
	10	1	ii. O.
	12	17	i. *e*.
	15	44	i. O.
31	10	17	iii. *e*.
	10	37	i. *t*.
	12	52	i. T.
	13	6	iii. E
	15	20	iii. *o*.
	17	59	iii. O

The satellites of Jupiter will be invisible from August 14 until October 12, Jupiter being too near the Sun. Satellites I. & II. *disappear* at eclipse on the *p* side in January and February. They *reappear* on th *f* side from March to June. They *reappear* on the *f* side in August, and *disappear* on the *p* side from October to December.

Satellites III. & IV. *disappear* and *reappear* at eclipse on the *p* side in January. They *disappear* o the *p* side in February, and both *disappear* and *reappear* on the *f* side from March to August. The *disappear* and *reappear* on the *p* side from October to December.

EPHEMERIS OF JUPITER'S VTH SATELLITE.

1897.	P+90°.	*a*.	*b*.	*l*−L.	Time of greatest Elongation W.	
	°	"	"	°	h	m
Jan. 1	114·41	51·97	1·17	228·36	13	20
3	114·40	52·26	1·19	233·79	13	9
5	114·40	52·55	1·20	239·23	12	59
7	114·39	52·84	1·21	244·68	12	48
9	114·38	53·12	1·22	250·14	12	37
11	114·37	53·39	1·23	255·61	12	26
13	114·35	53·65	1·24	261·09	12	15
15	114·34	53·91	1·25	266·58	12	4
17	114·32	54·16	1·26	272·07	11	53
19	114·30	54·41	1·27	277·57	11	42
21	114·28	54·64	1·28	283·08	11	31
23	114·26	54·87	1·28	288·60	11	20
25	114·24	55·08	1·29	294·12	11	9
27	114·21	55·29	1·29	299·65	10	58
29	114·18	55·48	1·30	305·18	10	47
31	114·16	55·66	1·30	310·71	10	36
Feb. 2	114·13	55·83	1·30	316·25	10	25
4	114·10	55·98	1·30	321·80	10	14
6	114·07	56·12	1·30	327·34	10	3
8	114·03	56·25	1·30	332·89	9	52
10	114·00	56·36	1·30	338·43	9	41
12	113·96	56·45	1·30	343·98	9	30
14	113·93	56·53	1·30	349·53	9	19
16	113·89	56·60	1·29	355·07	9	8
18	113·85	56·65	1·29	0·61	8	57
20	113·81	56·68	1·28	6·15	8	46
22	113·77	56·70	1·28	11·68	8	35
24	113·73	56·70	1·27	17·21	8	24
26	113·69	56·68	1·26	22·73	8	13
28	113·65	56·65	1·25	28·25	8	2
Mar. 2	113·61	56·60	1·24	33·76	7	51
4	113·56	56·54	1·23	39·26	7	40
6	113·52	56·46	1·22	44·75	7	29
8	113·48	56·37	1·21	50·24	7	18
10	113·44	56·26	1·20	55·72	7	7
12	113·40	56·14	1·19	61·18	6	56
14	113·36	56·00	1·18	66·63	6	45
16	113·32	55·85	1·16	72·08	18	32
18	113·28	55·69	1·15	77·51	18	21
20	113·24	55·46	1·14	82·92	18	10
22	113·21	55·32	1·13	88·33	17	59
24	113·17	55·12	1·11	93·72	17	49
26	113·14	54·91	1·10	99·10	17	38
28	113·11	54·69	1·08	104·46	17	27
30	113·08	54·46	1·07	109·81	17	17

The differences between successive values of $l-L$ for the interval of two days vary between 1445°·03 and 1445°·55. The values are to be interpolated for the times of the observations, when the corresponding computed coordinates will be

$x=a \sin (l-L)$ in pos. angle P±50°
$y=b \cos (l-L)$ „ „ P.

EPHEMERIS OF JUPITER'S VTH SATELLITE (*continued*).

1897.	P+90°.	a.	b.	l–L.	Time of greatest Elongation W.
	°	″	″	°	h m
Apr. 1 ……	113·05	54·22	1·06	115·15	17 6
3 ……	113·02	53·97	1·04	120·47	16 55
5 ……	113·00	53·71	1·03	125·77	16 45
7 ……	112·97	53·45	1·02	131·06	16 34
9 ……	112·95	53·18	1·00	136·34	16 24
11 ……	112·94	52·91	0·99	141·60	16 13
13 ……	112·92	52·63	0·98	146·84	16 3
15 ……	112·90	52·35	0·97	152·08	15 52
17 ……	112·89	52·06	0·96	157·29	15 42
19 ……	112·88	51·77	0·94	162·49	15 32
21 ……	112·88	51·48	0·93	167·68	15 21
23 ……	112·87	51·18	0·92	172·85	15 11
25 ……	112·87	50·88	0·91	178·01	15 1
27 ……	112·87	50·59	0·90	183·16	14 51
29 ……	112·87	50·29	0·89	188·29	14 40
May. 1 ……	112·88	49·99	0·88	193·40	14 30
3 ……	112·88	49·69	0·87	198·50	14 20
5 ……	112·89	49·39	0·87	203·59	14 10
7 ……	112·90	49·09	0·86	208·67	14 0
9 ……	112·91	48·79	0·85	213·73	13 50
11 ……	112·93	48·50	0·84	218·78	13 40
13 ……	112·95	48·21	0·84	223·82	13 30
15 ……	112·97	47·92	0·83	228·85	13 20

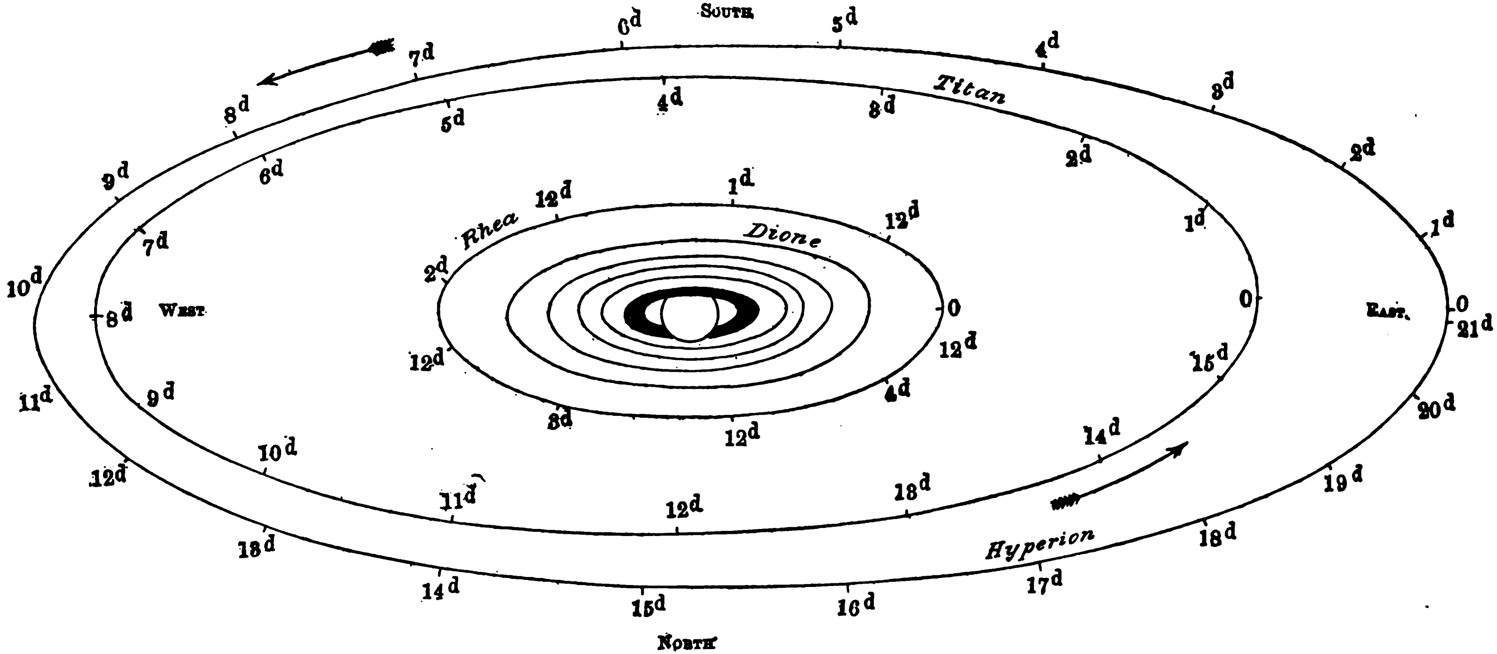
CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 14h	Feb. 13h	March. 12h	April. 11h	May. 11h	June. 10h	July. 9h	Aug. 8h 30m	Oct. 17h	Nov. 17h	Dec. 16h	Day.
1	421O3	423O1	2O134	31O24	4213O	4O213	24O31	312O4		♃431O	4312O	1
2	4O123	4321O	231O4	32O14	4O13●	421O3	431O2	23O14		342O1	43O12	2
3	♃4O3●	♃43O2	3O124	21O34	1O423	43O21	43O21	1O432		341O2	41O23	3
4	4321O	♃43O1	31O24	O1234	2O134	341O2	4321O	4O123		O1432	42O13	4
5	34O21	421O3	2O14●	O234●	231O4	342O1	42O1●	421O3		2O43●	41O23	5
6	314O2	4O213	42O13	21O34	♃3O24	2O4●●	41O23	42O31		21O34	♃43O2	6
7	2O314	41O23	41O23	32O14	3O124	1O234	42O13	431O2		O3124	324O1	7
8	21O34	♃24O1	♃4O13	314O2	231O4	O1234	42O3●	♃431O		31O24	321O4	8
9	O1234	321O4	4231O	♃43O1	2O134	21O34	31O2●	423O1		32O14	3O124	9
10	O234●	♃3O24	43O21	421O3	1O243	32O14	3O124	41O32		31O24	1O234	10
11	231O4	3O124	431O2	4O213	♃2O13	31O24	321O4	4O123		O3124	2O134	11
12	3O214	21O34	423O1	41O23	♃241O	♃3O14	2O14●	2O134	432O1	214O3	1O234	12
13	31O24	O2134	42O3●	♃42O3	43O12	231O4	1O234	31O24	4O2●●	♃42O3	O3124	13
14	2O41●	1O234	1O23●	432O1	43O2●	♃4O23	♃O134		41O23	4O132	32O4●	14
15	214O3	2O314	O2134	341O2	4231O	4O123	2O34●		42O13	431O2	321O4	15
16	4O123	321O4	213O4	♃3O1●	42O13	421O3	31O24		41O3●	432O1	34O12	16
17	41O23	34O12	3O214	21O4●	41O23	423O1	3O412		43O12	431O●	41O23	17
18	♃423O	43O2●	31O24	O134●	♃4O13	431O2	3421O		312O4	4O12●	42O13	18
19	43O1●	421O3	23O14	1O234	214O3	43O21	423O1		32O14	421O3	41O3●	19
20	431O2	4O13●	21O34	2O134	3O21●	4231O	41O23		31O24	2O13●	4O312	20
21	42O1●	41O23	♃O243	32O4●	3O24●	4O13●	4O213		♃O234	O423●	432O●	21
22	42●O3	42O31	♃O213	31O24	321O4	O23●●	421O3		2O134	31O24	♃432O	22
23	O4123	4321O	4213O	3O214	2O134	21O43	♃43O●		1O34●	32O14	34O12	23
24	1O234	34O12	43O1●	21O4●	1O234	♃2O14	43O12		3O124	31O4●	1O432	24
25	23O14	3O412	431O2	4O13●	O2134	31O24	3421O		312O4	O124●	2O143	25
26	32O4●	21O34	432O1	41O23	21O34	3O214	23O1●		♃32O1	12O34	1O34●	26
27	31O24	2O134	421O3	42O13	3O14●	231O4	1O234		431O2	2O134	O1324	27
28	32O14	1O234	♃4O23	423O●	314O2	O134●	O2134		4O123	O243●	312O4	28
29	21O34		4O23●	431O2	♃432O	O243●	21O34		42O13	341O2	32O14	29
30	O2143		♃214O	43O12	42O1●	21O43	♃O14●		412O3	342O1	3O124	30
31	1O423		3O214		41O23		3O24●		♃4O12		1O24●	31

The circle (O) represents Jupiter ; ♃ signifies that the satellite is on the disk ;
● signifies that the satellite is behind the disk or in the shadow.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

SATELLITES OF SATURN.

East Elongations.

MIMAS.

January.		February.		March.		April.		May.		June.		July.		August.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
1	16·1	1	18·3	1	2·3	1	4·6	1	8·1	1	10·4	1	14·1	1	16·6
2	14·7	2	16·9	2	1·0	2	3·2	2	6·7	2	9·0	2	12·7	2	15·2
3	13·3	3	15·6	2	23·6	3	1·8	3	5·3	3	7·6	3	11·3	3	13·8
4	11·9	4	14·2	3	22·1	4	0·4	4	3·9	4	6·2	4	9·9	4	12·4
5	10·5	5	12·8	4	20·7	4	23·0	5	2·6	5	4·8	5	8·5	5	11·0
6	9·1	6	11·4	5	19·3	5	21·6	6	1·2	6	3·4	6	7·1	6	9·6
7	7·7	7	10·0	6	17·9	6	20·2	6	23·8	7	2·0	7	5·7	7	8·2
8	6·3	8	8·6	7	16·5	7	18·9	7	22·4	8	0·6	8	4·3	8	6·8
9	4·9	9	7·3	8	15·1	8	17·5	8	21·1	8	23·2	9	3·0	9	5·4
10	3·5	10	5·9	9	13·7	9	16·1	9	19·7	9	21·8	10	1·6	10	4·0
11	2·1	11	4·6	10	12·3	10	14·7	10	18·3	10	20·5	11	0·2	11	2·6
12	0·7	12	3·2	11	11·0	11	13·3	11	16·9	11	19·2	11	22·8	12	1·2
12	23·3	13	1·8	12	9·6	12	11·9	12	15·6	12	17·8	12	21·4	12	23·8
13	21·9	14	0·4	13	8·2	13	10·5	13	14·2	13	16·4	13	20·0	13	22·5
14	20·5	14	23·0	14	6·8	14	9·1	14	12·8	14	15·0	14	18·6	14	21·1
15	19·1	15	21·6	15	5·4	15	7·7	15	11·4	15	13·7	15	17·3	15	19·8
16	17·7	16	20·2	16	4·0	16	6·3	16	10·0	16	12·3	16	16·0	16	18·4
17	16·4	17	18·8	17	2·6	17	4·9	17	8·6	17	10·9	17	14·7	17	17·0
18	15·0	18	17·4	18	1·2	18	3·8	18	7·2	18	9·5	18	13·3	18	15·7
19	13·6	19	16·1	18	23·9	19	2·3	19	5·9	19	8·1	19	11·9	19	14·4
20	12·3	20	14·7	19	22·5	20	0·9	20	4·5	20	6·8	20	10·5	20	13·0
21	10·9	21	13·3	20	21·2	20	23·5	21	3·1	21	5·4	21	9·1		
22	9·5	22	11·9	21	19·8	21	22·1	22	1·7	22	4·0	22	7·7		
23	8·1	23	10·6	22	18·4	22	20·7	23	0·3	23	2·6	23	6·3		
24	6·7	24	9·2	23	17·0	23	19·3	23	22·9	24	1·2	24	4·9		
25	5·3	25	7·8	24	15·6	24	17·9	24	21·5	24	23·8	25	3·5		
26	3·9	26	6·4	25	14·3	25	16·5	25	20·1	25	22·5	26	2·2		
27	2·6	27	5·0	26	12·9	26	15·1	26	18·8	26	21·1	27	0·8		
28	1·2	28	3·7	27	11·5	27	13·7	27	17·4	27	19·7	27	23·4		
28	23·8			28	10·1	28	12·3	28	16·0	28	18·3	28	22·0		
29	22·4			29	8·7	29	10·9	29	14·6	29	16·9	29	20·6		
30	21·1			30	7·4	30	9·5	30	13·2	30	15·5	30	19·3		
31	19·7			31	6·0			31	11·8			31	17·9		

ENCELADUS.

January.		February.		March.		April.		May.		June.		July.		August.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
21	8·0	1	7·2	2	1·8	1	5·2	1	8·5	1	20·6	1	23·9	1	3·4
22	16·9	2	16·1	3	10·7	2	14·1	2	17·3	3	5·5	3	8·8	2	12·3
24	1·8	4	0·9	4	19·6	3	22·9	4	2·2	4	14·3	4	17·7	3	21·2
25	10·7	5	9·8	6	4·4	5	7·8	5	11·1	5	23·2	6	2·6	5	6·1
26	19·6	6	18·7	7	13·3	6	16·7	6	20·0	7	8·1	7	11·5	6	15·0
28	4·5	8	3·6	8	22·2	8	1·6	8	4·8	8	17·0	8	20·3	7	23·9
29	13·4	9	12·5	10	7·1	9	10·4	9	13·7	10	1·9	10	5·2	9	8·7
30	22·3	10	21·4	11	16·0	10	19·3	10	22·6	11	10·7	11	14·1	10	17·6
		12	6·3	13	0·9	12	4·2	12	7·5	12	19·6	12	23·0	12	2·5
		13	15·2	14	9·7	13	13·1	13	16·3	14	4·5	14	7·9	13	11·4
		15	0·1	15	18·6	14	22·0	15	1·2	15	13·4	15	16·8		
		16	8·9	17	3·5	16	6·8	16	10·1	16	22·2	17	1·7		
		17	17·8	18	12·4	17	15·7	17	19·0	18	7·1	18	10·5		
		19	2·7	19	21·3	19	0·6	19	3·8	19	16·0	19	19·4		
		20	11·6	21	6·1	20	9·5	20	12·7	21	0·9	21	4·3		
		21	20·5	22	15·0	21	18·3	21	21·6	22	9·8	22	13·2		
		23	5·4	23	23·9	23	3·2	23	6·5	23	18·6	23	22·1		
		24	14·3	25	8·8	24	12·1	24	15·3	25	3·5	25	7·0		
		25	23·1	26	17·7	25	21·0	26	0·2	26	12·4	26	15·9		
		27	8·0	28	2·5	27	5·8	27	9·1	27	21·3	28	0·7		
		28	16·9	29	11·4	28	14·7	28	18·0	29	6·2	29	9·6		
				30	20·3	29	23·6	30	2·8	30	15·0	30	18·5		
								31	11·7						

East Elongations.

Tethys.

January.	February.	March.	April.	May.	June.	July.	August.
d h	d h	d h	d h	d h	d h	d h	d h
1 20·2	1 1·2	1 8·9	2 11·0	2 15·7	1 20·3	2 1·0	1 5·9
3 17·5	2 22·5	3 6·2	4 8·3	4 12·9	3 17·6	3 22·3	3 3·2
5 14·8	4 19·8	5 3·5	6 5·6	6 10·2	5 14·8	5 19·6	5 0·5
7 12·1	6 17·1	7 0·8	8 2·9	8 7·5	7 12·1	7 16·9	6 21·8
9 9·4	8 14·5	8 22·1	10 0·2	10 4·8	9 9·4	9 14·2	8 19·1
11 6·7	10 11·8	10 19·4	11 21·5	12 2·1	11 6·7	11 11·5	10 16·4
13 4·0	12 9·1	12 16·7	13 18·8	13 23·4	13 4·0	13 8·8	12 13·8
15 1·3	14 6·4	14 14·0	15 16·0	15 20·7	15 1·3	15 6·1	
16 22·6	16 3·7	16 11·3	17 13·3	17 18·0	16 22·6	17 3·4	
18 20·0	18 1·0	18 8·6	19 10·6	19 15·2	18 19·9	19 0·7	
20 17·3	19 22·3	20 5·9	21 7·9	21 12·5	20 17·2	20 22·0	
22 14·6	21 19·6	22 3·2	23 5·2	23 9·8	22 14·5	22 19·3	
24 11·9	23 17·0	24 0·5	25 2·5	25 7·1	24 11·8	24 16·6	
26 9·3	25 14·3	25 21·8	26 23·8	27 4·4	26 9·1	26 13·9	
28 6·6	27 11·6	27 19·1	28 21·1	29 1·7	28 6·4	28 11·3	
30 3·9		29 16·4	30 18·4	30 23·0	30 3·7	30 8·6	
		31 13·7					

Dione.

January.	February.	March.	April.	May.	June.	July.	August.
d h	d h	d h	d h	d h	d h	d h	d h
4 1·2	3 4·0	2 12·9	1 15·4	1 17·5	3 13·3	3 15·6	2 18·1
6 18·9	5 21·7	5 6·6	4 9·0	4 11·2	6 7·0	6 9·2	
9 12·6	8 15·4	8 0·3	7 2·7	7 4·8	9 0·6	9 2·9	
12 6·3	11 9·1	10 18·0	9 20·3	9 22·5	11 18·3	11 20·6	
15 0·0	14 2·8	13 11·7	12 14·0	12 16·1	14 11·9	14 14·3	
17 17·7	16 20·5	16 5·3	15 7·6	15 9·8	17 5·6	17 7·9	
20 11·4	19 14·2	18 23·0	18 1·3	18 3·4	19 23·3	20 1·6	
23 5·1	22 7·9	21 16·7	20 18·9	20 21·1	22 16·9	22 19·3	
25 22·8	25 1·6	24 10·3	23 12·6	23 14·7	25 10·6	25 13·0	
28 16·5	27 19·2	27 4·0	26 6·3	26 8·4	28 4·2	28 6·7	
31 10·3		29 21·7	28 23·9	29 2·0	30 21·9	31 0·4	
				31 19·7			

Rhea.

January.	February.	March.	April.	May.	June.	July.	August.
d h	d h	d h	d h	d h	d h	d h	d h
4 15·3	5 6·8	4 9·6	5 0·3	2 2·3	2 16·4	4 6·8	4 21·7
9 3·8	9 19·3	8 22·0	9 12·6	6 14·6	7 4·8	8 19·2	
13 16·3	14 7·8	13 10·4	14 1·0	11 2·9	11 17·1	13 7·6	
18 4·8	18 20·3	17 22·8	18 13·3	15 15·2	16 5·4	17 20·0	
22 17·3	23 8·7	22 11·2	23 1·6	20 3·5	20 17·8	22 8·4	
27 5·8	27 21·1	26 23·5	27 13·9	24 15·8	25 6·1	26 20·9	
31 18·3		31 11·9		29 4·1	29 18·5	31 9·3	

Titan.

January.	February.	March.	April.	May.	June.	July.
d h	d h	d h	d h	d h	d h	d h
13 4·5	14 3·1	2 2·5	2 23·3	4 18·6	5 13·3	7 8·9
29 3·8		18 1·1	18 21·1	20 16·0	21 11·0	

East Elongations.

HYPERION.

January.	February.	March.	April.	May.	June.	July.	August.
d h	d h	d h	d h	d h	d h	d h	d h
19 5·7	9 16·8	3 2·3 24 10·0	14 16·1	5 20·9 27 0·7	17 4·3	8 8·1 29 12·6	19 17·8

IAPETUS.

	d h	d h	d h
Inf. Conjunction ...	Mar. 1 21·4,	May 19 11·0,	Aug. 6 1·8.
W. Elongation......	Jan. 2 0·8,	Mar. 23 2·8,	June 9 7·9.
Sup. Conjunction...	Jan. 22 1·6,	Apr. 11 14·7,	June 28 17·4.
E. Elongation	Feb. 11 14·7,	May 1 14·2,	July 18 22·2.

Apparent Elements of Saturn's Rings.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 0	2 24·3	35·28	14·45	24 10·7 N.	23 15·3 N.
20	2 38·1	36·17	14·97	24 27·1 N.	23 24·1 N.
Feb. 9	2 47·8	37·31	15·53	24 36·4 N.	23 32·6 N.
Mar. 1	2 52·7	38·58	16·09	24 38·7 N.	23 41·0 N.
21	2 52·2	39·85	16·58	24 34·8 N.	23 49·2 N.
April 10	2 46·7	40·95	16·93	24 25·4 N.	23 57·2 N.
30	2 37·3	41·67	17·08	24 12·0 N.	24 5·1 N.
May 20	2 25·7	41·89	17·00	23 56·7 N.	24 12·7 N.
June 9	2 14·4	41·55	16·71	23 42·5 N.	24 20·3 N.
29	2 5·5	40·74	16·28	23 32·8 N.	24 27·6 N.
July 19	2 0·7	39·60	15·79	23 30·0 N.	24 34·8 N.
Aug. 8	2 0·9	38·33	15·34	23 35·4 N.	24 41·8 N.
28	2 6·2	37·08	14·97	23 48·5 N.	24 48·6 N.
Sept. 17	2 16·2	35·97	14·71	24 7·9 N.	24 55·2 N.
Oct. 7	2 29·9	35·09	14·56	24 30·8 N.	25 1·6 N.
27	2 46·5	34·48	14·53	24 54·9 N.	25 7·8 N.
Nov. 16	3 4·7	34·19	14·61	25 17·8 N.	25 14·0 N.
Dec. 6	3 23·2	34·21	14·79	25 37·4 N.	25 19·8 N.
26	3 40·8	34·55	15·08	25 52·7 N.	25 25·5 N.
31	3 44·9	34·68	15·17	25 55·7 N.	25 26·9 N.

SATELLITES OF URANUS.

North Elongations.

ARIEL.

February.		March.		April.		May.		June.		July.		August.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
1	18·8	1	12·2	3	6·6	1	0·1	2	18·7	3	0·8	2	6·8
4	7·3	4	0·7	5	19·1	3	12·6	5	7·2	5	13·3	4	19·3
6	19·8	6	13·2	8	7·6	6	1·1	7	19·8	8	1·9	7	7·8
9	8·3	9	1·7	10	20·1	8	13·7	10	8·3	10	14·4	9	20·3
11	20·8	11	14·2	13	8·6	11	2·2	12	20·8	13	2·9	12	8·8
14	9·3	14	2·7	15	21·1	13	14·7	15	9·3	15	15·4	14	21·3
16	21·8	16	15·1	18	9·6	16	3·2	17	21·8	18	3·9		
19	10·3	19	3·6	20	22·1	18	15·7	20	10·3	20	16·4		
21	22·8	21	16·1	23	10 6	21	4·2	22	22·8	23	4·9		
24	11·3	24	4·6	25	23·1	23	16·7	25	11·3	25	17·4		
26	23·8	26	17·1	28	11·6	26	5·2	27	23·8	28	5·9		
		29	5·6			28	17·7	30	12·3	30	18·3		
		31	18·1			31	6·2						

UMBRIEL.

February.		March.		April.		May.		June.		July.		August.		September.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
3	12·3	4	12·4	2	12·7	1	13·2	3	17·3	2	17·9	4	21·7	......	
7	15·8	8	15·9	6	16·2	5	16·6	7	20·8	6	21·4	9	1·2	......	
11	19·2	12	19·3	10	19·6	9	20·2	12	0·4	11	0·9	13	4·7	......	
15	22·6	16	22·8	14	23·2	13	23·7	16	3·9	15	4·4	17	8·1	......	
20	2·1	21	2·3	19	2·7	18	3·3	20	7·4	19	7·8	21	11·6	......	
24	5·5	25	5·7	23	6·2	22	6·8	24	10·9	23	11·3	25	15·0	......	
28	9·0	29	9·2	27	9·7	26	10·3	28	14·4	27	14·8				
						30	13·8			31	18·3				

TITANIA.

January.		February.		March.		April.		May.		June.		July.		August.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
31	20·1	9	12·8	7	15·4	2	18·5	7	15·1	2	18·6	7	15·1	2	18·2
		18	5·6	16	8·4	11	11·6	16	8·2	11	11·8	16	8·2	11	11·1
		26	22·5	25	1·4	20	4·7	25	1·4	20	5·0	25	1·2	20	4·0
						28	21·9			28	22·0			28	20·9

OBERON.

March.		April.		May.		June.		July.	
d	h	d	h	d	h	d	h	d	h
1	15·5	11	1·5	8	0·8	4	0·2	14	10·7
15	2·7	24	13·1	21	12·4	17	11·9	27	22·0
28	14·0					30	23·3		

Position-angle and distance of the apse of Oberon:—February 18, 348°·0, 43″·1; May 17, 350°·8, 45″·4; August 13, 353°·7, 43″·1.

SATELLITE OF NEPTUNE.

North-east Elongations.

January.		February.		March.		September.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
2	0·6	6	7·2	1	19·5	5	18·7	5	3·8	3	13·2	2	22·7
7	21·7	12	4·3	7	16·6	11	15·7	11	0·9	9	10·3	8	19·8
13	18·8	18	1·4	13	13·6	17	12·7	16	21·9	15	7·4	14	16·9
19	15·9	23	22·5	19	10·7	23	9·7	22	19·0	21	4·5	20	14·1
25	13·0			25	7·7	29	6·8	28	16·1	27	1·6	26	11·2
31	10·1												

Position-angle and distance of the apse:—March 6, 252°·8, 16″·4; Aug. 29, 258°·0, 16″·3; December 3, 257°·0, 16″·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1897.	P.	D.	L.	1897.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 1 32	−3 20	86 42	July 5	− 0 28	+3 34	164 50
6	− 0 54	3 53	20 52	10	+ 1 49	4 5	98 40
11	3 19	4 25	315 2	15	4 4	4 35	32 29
16	5 41	4 54	249 10	20	6 16	5 2	326 20
21	7 59	5 22	183 21	25	8 24	5 27	260 11
26	10 12	5 46	117 30	30	10 29	5 49	194 4
31	12 18	6 8	51 42	Aug. 4	12 27	6 10	127 57
Feb. 5	14 18	6 28	345 52	9	14 20	6 28	61 50
10	16 10	6 43	280 1	14	16 7	6 43	355 43
15	17 54	6 56	214 11	19	17 46	6 55	289 39
20	19 30	7 6	148 19	24	19 18	7 5	223 36
25	20 56	7 12	82 28	29	20 43	7 11	157 33
Mar. 2	22 13	7 15	16 37	Sept. 3	21 59	7 15	91 30
7	23 21	7 14	310 43	8	23 6	7 15	25 27
12	24 18	7 11	244 49	13	24 5	7 12	319 27
17	25 6	7 3	178 54	18	24 54	7 6	253 27
22	25 43	6 53	112 58	23	25 33	6 57	187 28
27	26 9	6 40	47 4	28	26 2	6 44	121 28
Apr. 1	26 25	6 24	341 5	Oct. 3	26 21	6 29	55 29
6	26 30	6 5	275 8	8	26 30	6 11	349 31
11	26 24	5 43	209 6	13	26 27	5 50	283 34
16	26 7	5 19	143 6	18	26 13	5 26	217 38
21	25 39	4 53	77 4	23	25 47	5 00	151 41
26	25 00	4 24	10 59	28	25 10	4 31	85 44
May 1	24 10	3 54	304 54	Nov. 2	24 20	4 00	19 49
6	23 9	3 23	238 48	7	23 19	3 28	313 55
11	21 57	2 49	172 42	12	22 7	2 54	247 59
16	20 35	2 15	106 34	17	20 42	2 18	182 4
21	19 4	1 40	40 26	22	19 7	1 41	116 9
26	17 23	1 5	334 16	27	17 22	1 4	50 16
31	15 35	−0 29	268 5	Dec. 2	15 26	+0 25	344 23
June 5	13 39	+0 8	201 55	7	13 22	−0 13	278 29
10	11 37	0 44	135 44	12	11 11	0 51	212 36
15	9 28	1 20	69 34	17	8 53	1 29	146 43
20	7 17	1 55	3 22	22	6 31	2 7	80 51
25	5 2	2 29	297 11	27	4 6	2 43	15 1
30	− 2 45	+3 3	231 00	32	+ 1 39	−3 18	309 11

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 22′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1897·0.

No.	Star.	R.A.	Dec.
		h m s	° ′
1.	V Sculptoris	0 3 24	−39 48·2
2.	S Sculptoris	0 10 10	−32 37·1
3.	T Andromedæ	0 17 1	+26 25·4
4.	T Cassiopeiæ	0 17 39	+55 13·3
5.	S Toucani	0 18 15	−62 15·0
6.	R Andromedæ	0 18 35	+38 0·4
7.	S Ceti	0 18 49	− 9 53·9
8.	U Cassiopeiæ	0 40 36	+47 41·6
9.	W Cassiopeiæ	0 48 48	+58 0·3
10.	U Cephei	0 53 9	+81 19·2
11.	U Sculptoris	1 6 41	−30 39·7
11*a*	U Andromedæ	1 9 37	+40 10·5
12.	S Cassiopeiæ	1 12 5	+72 4·1
13.	S Piscium	1 12 11	+ 8 23·3
14.	U Piscium	1 17 32	+12 19·7
15.	R Sculptoris	1 22 14	−33 4·5
16.	R Piscium	1 25 19	+ 2 21·0
17.	U Persei	1 52 44	+54 19·3
18.	S Arietis	1 59 6	+12 2·0
19.	R Arietis	2 10 15	+24 34·6
20.	*o* Ceti	2 14 9	− 3 26·7
21.	R Ceti	2 20 46	− 0 38·6
22.	U Ceti	2 28 47	−13 36·0
23.	R Trianguli	2 30 48	+33 48·9
24.	T Arietis	2 42 35	+17 4·8
25.	V Persei	2 43 2	+56 33·3
26.	R Horologii	2 49 36	−50 22·0
27.	Algol	3 1 28	+40 33·6
28.	U Arietis	3 5 20	+14 24·6
29.	R Persei	3 23 29	+35 19·3
30.	T Eridani	3 50 49	−24 20·1
31.	λ Tauri	3 54 58	+12 12·0
32.	W Tauri	4 22 9	+15 52·5
33.	R Tauri	4 22 39	+ 9 56·0
34.	S Tauri	4 23 34	+ 9 43·1
35.	T Camelopardi	4 30 4	+65 56·4
36.	R Reticuli	4 32 28	−63 14·6
37.	V Tauri	4 46 4	+17 21·9
38.	R Orionis	4 53 25	+ 7 58·5
39.	R Leporis	4 54 55	−14 57·7
40.	T Leporis	5 0 27	−22 2·7
41.	V Orionis	5 0 38	+ 3 57·7
42.	S Pictori	5 8 13	−48 38·0
43.	R Aurigæ	5 8 59	+53 28·2
44.	T Columbæ	5 15 31	−33 48·9
45.	S Orionis	5 23 55	− 4 46·5
46.	S Camelopardi	5 29 54	+68 44·5
47.	U Aurigæ	5 35 21	+31 58·6
48.	R Columbæ	5 46 33	−29 13·2
49.	U Orionis	5 49 42	+20 9·4
50.	η Geminorum	6 8 40	+22 32·2
51.	V Aurigæ	6 16 5	+47 42·8
52.	V Monocerotis	6 17 32	− 2 8·7
53.	T Monocerotis	6 19 40	+ 7 8·5
54.	S Monocerotis	6 35 19	+ 9 59·3
55.	W Monocerotis	6 47 21	− 7 1·4
56.	R Lyncis	6 52 48	+55 28·5
57.	ζ Geminorum	6 58 0	+20 43·3
58.	R Geminorum	7 1 9	+22 51·8
58*a*	V Canis Min.	7 1 23	+ 9 1·8
59.	R Canis Min.	7 3 3	+10 11·1
60.	L^2 Puppis	7 10 23	−44 28·5
61.	R Canis Maj.	7 14 48	−16 12·1
62.	V Geminorum	7 17 24	+13 17·4
63.	U Monocerotis	7 25 53	− 9 33·7
64.	S Canis Min.	7 27 8	+ 8 32·3
65.	T Canis Min.	7 28 16	+11 57·9
66.	U Canis Min.	7 35 45	+ 8 37·2
67.	S Geminorum	7 36 52	+23 41·6
68.	T Geminorum	7 43 7	+23 59·4
69.	U Geminorum	7 49 0	+22 16·3
70.	U Puppis	7 56 0	−12 33·3
71.	R Cancri	8 10 53	+12 2·6
72.	V Cancri	8 15 51	+17 36·7
73.	V Carini	8 26 38	−59 46·5
74.	U Cancri	8 29 53	+19 15·1
75.	T Velorum	8 34 20	−47 0·1
76.	S Cancri	8 38 3	+19 24·3
77.	R Mali	8 41 10	−27 49·5
78.	S Hydræ	8 48 12	+ 3 27·4
79.	T Hydræ	8 50 39	− 8 44·9
80.	T Cancri	8 50 47	+20 14·6
81.	W Carini	9 19 9	−55 31·2
82.	S Antliæ	9 27 48	−28 10·4
83.	N Velorum	9 28 6	−56 34·8
84.	S Velorum	9 29 20	−44 45·1
85.	R Carini	9 29 39	−62 20·0
86.	X Hydræ	9 30 35	−14 13·9
87.	R Leo. Min.	9 39 25	+34 59·2
88.	R Leonis	9 42 1	+11 54·4
89.	*l* Carini	9 42 25	−62 2·0
90.	V Leonis	9 54 18	+21 45·2
91.	S Carini	10 6 5	−61 2·7
92.	R Ursæ Maj.	10 37 22	+69 19·0
93.	V Hydræ	10 46 37	−20 41·6
94.	W Leonis	10 48 12	+14 16·5
95.	U Carini	10 53 36	−59 10·9
96.	S Leonis	11 5 31	+ 6 1·2
97.	X Centauri	11 44 3	−41 10·9
98.	W Centauri	11 49 53	−58 40·8
99.	R Comæ	11 58 58	+19 21·5
100.	S Muscæ	12 7 14	−69 34·7
101.	T Virginis	12 9 20	− 5 27·8
102.	R Corvi	12 14 18	−18 40·9
103.	R Crucis	12 17 59	−61 3·5
104.	Y Virginis	12 28 34	− 3 51·2

Mean Places of Variable Stars for 1897·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
105.	T Ursæ Maj. ...	12 31 42	+60 3·3	160.	V Ophiuchi ...	16 21 0	−12 11·6
106.	R Virginis	12 33 7	+ 7 33·3	161.	U Herculis	16 21 15	+19 7·4
107.	R Muscæ.........	12 35 47	−68 50·5	162.	Y Scorpii	16 23 39	−19 12·9
108.	S Ursæ Maj. ...	12 39 26	+61 39·4	163.	T Ophiuchi......	16 27 50	−15 54·7
109.	U Virginis	12 45 52	+ 6 6·8	164.	S Ophiuchi......	16 28 20	−16 56·6
110.	S Crucis	12 48 16	−57 52·3	165.	R Aræ............	16 31 11	−56 47·2
111.	W Virginis......	13 20 43	− 2 50·7	166.	W Herculis......	16 31 34	+37 33·0
112.	V Virginis	13 22 29	− 2 38·3	167.	Y Herculis	16 31 52	+ 7 19·0
113.	R Hydræ	13 24 6	−22 44·9	168.	R Draconis ...	16 32 23	+66 58·1
114.	S Virginis	13 27 37	− 6 39·9	169.	S Herculis	16 47 12	+15 6·9
115.	Z Centauri	13 34 6	−31 6·7	170.	S^2 Scorpii	16 48 9	−44 56·0
116.	T Centauri	13 35 52	−33 4·6	171.	R^2 Scorpii	16 50 4	−30 24·9
117.	W Hydræ	13 43 13	−27 51·1	172.	R Ophiuchi......	17 1 51	−15 57·3
118.	R Canum Ven. .	13 44 32	+40 3·3	173.	W^2 Scorpii	17 8 6	−33 18·8
119.	R^2 Virginis	13 59 25	− 8 42·3	174.	U Ophiuchi......	17 11 18	+ 1 19·5
120.	Z Virginis	14 4 48	−12 48·9	175.	Z Ophiuchi......	17 14 19	+ 1 37·5
121.	R Centauri	14 9 9	−59 26·0	176.	U^2 Scorpii	17 34 53	−43 42·2
122.	Y Boötis	14 17 14	+20 16·6	177.	X Sagittarii ...	17 41 4	−27 47·4
123.	X Boötis	14 19 18	+16 47·3	178.	Y Ophiuchi......	17 47 7	− 6 7·1
124.	S Boötis	14 19 26	+54 16·8	179.	Z Herculis	17 53 28	+15 8·4
125.	S^2 Virginis	14 22 7	+ 5 8·4	180.	W Sagittarii ...	17 58 27	−29 35·2
126.	R Camelopardi .	14 25 21	+84 18·0	181.	T Herculis	18 5 12	+31 0·1
127.	V Boötis	14 25 36	+39 19·2	182.	Y Sagittarii......	18 15 19	−18 54·4
128.	R Boötis	14 32 39	+27 11·0	183.	V^2 Sagittarii ...	18 21 9	−33 22·7
129.	V Libræ	14 34 38	−17 12·8	184.	T Serpentis......	18 23 47	+ 6 13·7
130.	S Lupi............	14 46 30	−46 11·3	185.	U Sagittarii ...	18 25 49	−19 12·0
131.	U Boötis	14 49 34	+18 6·9	186.	X Ophiuchi ...	18 33 27	+ 8 44·3
132.	δ Libræ	14 55 28	− 8 6·6	187.	R Scuti	18 41 59	− 5 49·0
133.	T Triang. Austr.	15 0 6	−68 18·9	188.	β Lyræ	18 46 17	+33 14·6
134.	T Libræ	15 4 52	−19 37·6	189.	κ Pavonis	18 46 20	−67 21·7
135.	Y Libræ	15 6 14	− 5 37·3	190.	R Lyræ	18 52 12	+43 48·6
136.	R Triang. Austr.	15 10 33	−66 7·1	191.	R Coron. Austr.	18 54 56	−37 5·7
137.	U Coronæ	15 14 0	+32 1·4	192	R Aquilæ	19 1 25	+ 8 4·5
138.	S Libræ	15 15 28	−20 0·9	193.	V Lyræ	19 5 2	+29 29·7
139.	S Serpentis......	15 16 50	+14 41·0	194.	S Lyræ	19 8 59	+25 49·7
140.	S Coronæ	15 17 12	+31 44·3	195.	W Aquilæ	19 7 50	− 7 13·3
141.	S^2 Libræ.........	15 18 19	−22 32·6	196.	T Sagittarii ...	19 10 18	−17 9·1
142.	U^2 Libræ.........	15 27 31	−14 58·7	197.	R Sagittarii ...	19 10 39	−19 29·3
143.	X Libræ	15 30 16	−20 49·3	198.	S Sagittarii......	19 13 24	−19 12·7
144.	W Libræ.........	15 32 2	−15 50·0	199.	Z Sagittarii......	19 13 37	−21 6·9
145.	U Libræ	15 36 3	−20 50·9	200.	T Sagittæ	19 17 6	+17 27·5
146.	Z Libræ	15 40 31	−20 48·0	201.	U Aquilæ	19 23 49	− 7 15·3
147.	V Coronæ	15 45 51	+39 52·9	202.	R Cygni	19 34 3	+49 58·2
148.	R Serpentis......	15 45 57	+15 26·8	203.	T Pavonis	19 39 11	−72 1·1
149.	R Libræ	15 47 46	−15 55·8	204.	T^2 Cygni.........	19 40 44	+48 31·5
150.	R^2 Libræ.........	15 50 29	−18 0·1	205.	S Vulpeculæ ...	19 44 10	+27 1·8
151.	S Triang. Austr.	15 51 56	−63 28·9	206.	X Aquilæ.........	19 46 22	+ 4 12·1
152.	X Herculis	15 59 34	+47 31·3	207.	χ Cygni	19 46 37	+32 39·2
153.	Z Scorpii.........	15 59 57	−21 27·1	208.	η Aquilæ.........	19 47 14	+ 0 44·5
154.	R Herculis	16 1 35	+18 38·9	209.	R^2 Sagittarii ...	19 49 32	−29 27·5
155.	X Scorpii	16 2 29	−21 15·0	210.	S Sagittæ	19 51 20	+16 21·7
156.	W Scorpii	16 5 45	−19 52·1	211.	R^2 Aquilæ	19 52 15	− 2 11·5
157.	R Scorpii	16 11 30	−22 41·4	212.	Z Cygni	19 58 32	+49 45·4
158.	S Scorpii.........	16 11 32	−22 38·3	213.	S Cygni	20 3 20	+57 41·4
159.	W Ophiuchi ...	16 15 52	− 7 27·2	214.	R Capricorni ...	20 5 32	−14 34·5

Mean Places of Variable Stars for 1897·0 (continued).

No.	Star.	R.A.			Dec.	
		h	m	s	°	′
215.	S Aquilæ………	20	6	53	+15	18·9
216.	W Capricorni…	20	8	25	−22	17·5
217.	R Sagittæ ……	20	9	21	+16	24·8
218.	Z Aquilæ………	20	9	42	− 6	27·9
219.	R Delphini……	20	9	57	+ 8	46·6
220.	U Cygni ………	20	16	25	+47	34·1
221.	W Delphini……	20	32	59	+17	55·2
222.	R Microscopii…	20	33	47	−29	9·2
223.	V Cygni ………	20	37	59	+47	46·4
224.	S Delphini ……	20	38	20	+16	43·1
225.	X Cygni ………	20	39	22	+35	12·9
226.	T Delphini ……	20	40	35	+16	1·4
227.	W Aquarii ……	20	41	1	− 4	27·6
228.	V Aquarii ……	20	41	37	+ 2	3·6
229.	U Capricorni …	20	42	24	−15	9·7
230.	R² Cygni………	20	42	31	+44	29·5
231.	V Delphini……	20	43	5	+18	57·4
232.	T Aquarii ……	20	44	30	− 5	31·7
233.	U Pavonis ……	20	46	55	−63	5·9
234.	T Vulpeculæ …	20	47	6	+27	51·6
235.	Y Cygni ………	20	47	57	+34	16·2
236.	R Vulpeculæ …	20	59	49	+23	24·8
237.	V Capricorni …	21	1	37	−24	20·4
238.	X Capricorni …	21	2	39	−21	45·7
239.	T Cephei………	21	8	11	+68	4·3
240.	T Capricorni …	21	16	20	−15	35·6
241.	Y Capricorni …	21	28	45	−14	25·8
242.	W Cygni………	21	32	9	+44	54·9
243.	S Cephei ………	21	36	30	+78	9·6
244.	U² Cygni………	21	37	10	+53	57·5
245.	R Gruis ………	21	41	54	−47	22·8
246.	U Aquarii ……	21	57	42	−17	7·4
247.	S Piscis Austr. .	21	57	52	−28	32·9
248.	T Pegasi………	22	3	52	+12	2·2
249.	R Piscis Austr. .	22	12	9	−30	7·1
250.	X Aquarii ……	22	12	59	−21	24·9
251.	S Gruis ………	22	19	44	−48	57·7
252.	S Lacertæ ……	22	24	30	+39	47·3
253.	δ Cephei………	22	25	21	+57	53·3
254.	W Cephei ……	22	32	32	+57	53·7
255.	R Lacertæ ……	22	38	42	+41	49·9
256.	S Aquarii ……	22	51	35	−20	53·6
257.	R Pegasi………	23	1	29	+ 9	59·3
258.	V Cassiopeiæ …	23	7	15	+59	7·5
259.	S Pegasi ………	23	15	20	+ 8	21·4
260.	R Aquarii ……	23	38	30	−15	51·3
261.	V Cephei………	23	51	36	+82	37·1
262.	V Ceti…………	23	52	38	− 9	32·0
263.	R Cassiopeiæ …	23	53	10	+50	48·9
264.	W Ceti ………	23	56	51	−15	41·9

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	
°	′	″
0		0·0
10		10·2
20		21·0
30		33·3
35		40·4
40		48·4
45		57·7
50	1	8·7
52	1	13·8
54	1	19·3
56	1	25·4
58	1	32·1
60	1	39·7
61	1	43·8
62	1	48·2
63	1	52·8
64	1	57·8
65	2	3·2
66	2	8·9
67	2	15·2
68	2	21·9
69	2	29·3
70	2	37·3
71	2	46·1
72	2	55·8
73	3	6·6
74	3	18·6
75	3	32·1
76	3	47·4
77	4	4·9
78	4	25·0
79	4	48·5
80	5	16·2
81	5	49·3
82	6	29·6
83	7	19·7
84	8	23·3
85	9	46·5

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's Name refers to Catalogue on pp. 24–26.)

Note.—The times of minima only are given; the times of maxima may be found by adding the inte printed under the name of the star.

T Monocer. 53.

(7^{d} 22½^{h}.)

Jan.	May	Sept.
d h	d h	d h
22 16	10 17	22 18
Feb.	June	Oct.
18 16	6 17	19 19
Mar.	July	Nov.
17 16	3 18	15 19
	30 18	
Apr.	Aug.	Dec.
13 17	26 18	12 19

ζ Geminor. 57.

(5^{d} 0^{h}.)

Jan.	May	Oct.
d h	d h	d h
10 14	2 7	1 14
20 18	12 10	11 18
30 21	22 14	21 21
Feb.	June	Nov.
10 1	1 18	1 1
20 5		11 5
Mar.	Aug.	21 8
2 8	1 16	Dec.
12 12	11 20	1 12
22 16	21 23	11 16
Apr.	Sept.	21 19
1 20	1 3	31 23
11 23	11 7	
22 3	21 10	

W Virginis 111.

(8^{d} 5^{h}.)

Jan.	Feb.	Mar.
d h	d h	d h
1 7	4 20	11 9
18 13	22 2	28 15

W Virginis 111

(*continued*).

Apr.	July	Nov.
d h	d h	d h
14 22	10 6	8 4
May	27 13	25 11
2 4	Aug.	Dec.
19 11	13 19	12 17
June	31 2	30 0
5 17	Oct.	
23 0	4 15	
	21 22	

X Sagittar. 177.

(2^{d} 21^{h}.)

Feb.	May	Sept.
d h	d h	d h
2 6	18 10	7 15
9 6	25 11	14 15
16 7	June	21 15
23 7	1 11	28 16
Mar.	8 11	Oct.
2 7	15 11	5 16
9 7	22 12	12 16
16 8	29 12	19 17
23 8	July	26 17
30 8	6 12	Nov.
Apr.	13 13	2 17
6 9	20 13	9 17
13 9	27 13	16 18
20 9	Aug.	23 18
27 9	3 13	30 18
May	10 14	Dec.
4 10	17 14	7 19
11 10	24 14	14 19
	31 15	

Y Ophiuchi 178.

(6^{d} 5½^{h}.)

Jan.	Apr.	Aug.
d h	d h	d h
1 23	14 17	12 13
19 2	May	29 16
Feb.	1 20	Sept.
5 5	18 22	15 19
22 8	June	Oct.
Mar.	5 1	2 22
11 11	22 4	20 1
28 14	July	
	9 7	
	26 10	

W Sagittar. 180.

(3^{d} 0^{h}.)

Jan.	May	Sept.
d h	d h	d h
24 18	3 12	2 0
Feb.	11 2	9 14
1 9	18 16	17 5
8 23	26 7	24 19
16 13	June	Oct.
24 3	2 21	2 9
Mar.	10 11	9 23
3 18	18 1	17 14
11 8	25 16	25 4
18 22	July	Nov.
26 12	3 6	1 18
Apr.	10 20	9 9
3 3	18 10	16 23
10 17	26 1	24 13
18 7	Aug.	Dec.
25 22	2 15	2 3
	10 5	
	17 20	
	25 10	

Y Sagittar. 182.

(1^{d} 19^{h}.)

Jan.	May	Sept.
d h	d h	d h
23 22	13 14	6 1
29 16	19 9	11 20
Feb.	25 3	17 14
4 11	30 22	23 9
10 5	June	29 4
16 0	5 16	Oct.
21 18	11 11	4 22
27 13	17 6	10 17
Mar.	23 0	16 11
5 7	28 19	22 6
11 2	July	28 0
16 21	4 13	Nov.
22 15	10 8	2 19
28 10	16 2	8 13
Apr.	21 21	14 8
3 4	27 15	20 3
8 23	Aug.	25 21
14 17	2 10	Dec.
20 12	8 5	1 16
26 6	13 23	7 10
May	19 18	
2 1	25 12	
7 20	31 7	

U Sagittar. 185.

(2^{d} 23^{h}.)

Jan.	Mar.	Apr.
d h	d h	d h
22 12	4 0	20 5
29 6	10 17	26 23
Feb.	17 11	May
5 0	24 5	3 16
11 18	30 23	10 10
18 12	Apr.	17 4
25 6	6 17	23 22
	13 11	30 16

U Sagittar.

(*continued*)

June	Aug.
d h	d h
6 10	6 3
13 4	12 20
19 22	19 14
26 15	26 8
July	Sept.
3 9	2 2
10 3	8 20
16 21	15 14
23 15	22 8
30 9	29 2

β Lyræ 18

(Max. 3^{d} 5^{h}.

Secondary Mi

6^{d} 11^{h}.

Max. 9^{d} 16^{h}.

Jan.	May	S
d h	d h	d
5 4	1 10	7
18 2	14 8	2
31 0	27 6	
Feb.	June	C
12 22	9 4	3
25 20	22 2	16
		29
Mar.	July	N
10 18	5 0	11
23 16	17 22	24
	30 20	
Apr.	Aug.	D
5 14	12 18	6
18 12	25 16	19

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 201.

(2^d $4\frac{1}{2}^h$.)

Feb.		May		Sept.	
d	h	d	h	d	h
4	22	21	7	3	16
1	23	28	8	10	16
9	0	June		17	17
6	0	4	8	24	17
Mar.		11	9		
5	1	18	9	Oct.	
2	1	25	10	1	18
9	2	July		8	19
6	3	2	11	15	19
Apr.		9	11	22	20
2	3	16	12	29	20
9	4	23	12		
6	4	30	13	Nov.	
3	5	Aug.		5	21
0	5	6	13	12	22
May		13	14	19	22
7	6	20	15	26	23
4	7	27	15		

η AQUILÆ 208.

(2^d 6^h.)

Feb.		Mar.		Mar.	
d	h	d	h	d	h
7	19	1	8	22	21
5	0	8	12	30	1
2	4	15	17		

η AQUILÆ 208 (*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
6	5	1	8	2	15
13	9	8	12	9	19
20	14	15	16	16	23
27	18	22	21	24	4
		30	1	31	8
May		Aug.		Nov.	
4	22	6	5	7	12
12	2	13	9	14	16
19	7	20	14	21	21
26	11	27	18	29	1
June		Sept.		Dec.	
2	15	3	22	6	5
9	19	11	2	13	9
17	0	18	7	20	14
24	4	25	11	27	18

S SAGITTÆ 210.

(3^d 10^h.)

Jan.		Mar.		Apr.	
d	h	d	h	d	h
26	7	9	5	20	3
		17	14	28	12
Feb.		25	23		
3	16			May	
12	1	Apr.		6	21
20	10	3	8	15	6
28	19	11	17	23	15

S SAGITTÆ 210 (*continued*).

June		Aug.		Oct.	
d	h	d	h	d	h
1	0	7	2	4	18
9	10	15	11	13	4
17	19	23	20	21	13
26	4			29	22
				Nov.	
				7	7
July		Sept.		15	16
4	13	1	6	24	2
12	22	9	15	Dec.	
21	8	18	0	2	11
29	17	26	9	10	20

X CYGNI 225.

(6^d 19^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
11	9	6	2	14	4
27	19	22	11	30	13
Feb.		June		Oct.	
13	4	7	21	16	23
		24	6		
Mar.		July		Nov.	
1	13	10	15	2	8
17	22	27	0	18	17
Apr.		Aug.		Dec.	
3	8	12	10	5	2
19	17	28	19	21	12

T VULPEC. 234.

(1^d $9\frac{1}{2}^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	16	3	11	4	16
8	3	7	21	9	2
12	13	12	8	13	13
17	0	16	18	17	23
21	10	21	5	22	10
25	21	25	15	26	20
30	7	30	2		
Feb.		June		Oct.	
3	17	3	12	1	7
8	4	7	22	5	17
12	14	12	9	10	3
17	1	16	19	14	14
21	11	21	6	19	0
25	22	25	16	23	11
		30	3	27	21
Mar.		July		Nov.	
2	8	4	13	1	8
6	19	9	0	5	18
11	5	13	10	10	5
15	16	17	21	14	15
20	2	22	7	19	2
24	13	26	18	23	12
28	23	31	4	27	23
Apr.		Aug.		Dec.	
2	10	4	15	2	9
6	20	9	1	6	20
11	6	13	11	11	6
15	17	17	22	15	16
20	3	22	8	20	3
24	14	26	19	24	13
29	0	31	5	29	0

δ CEPHEI 253.

(1^d $14\frac{1}{2}^h$.)

Jan.		May		Sep	
d	h	d	h	d	
2	1	5	11	5	2
7	10	10	20	11	
12	18	16	5	16	1
18	3	21	13	22	
23	12	26	22	27	
28	21				
		June		Oct	
Feb.		1	7	2	1
3	6	6	16	8	
8	14	12	0	13	1
13	23	17	9	18	1
19	8	22	18	24	
24	17	28	3	29	1
Mar.		July		Nov	
2	1	3	12	3	2
7	10	8	20	9	
12	19	14	5	14	1
18	4	19	14	20	
23	13	24	23	25	
28	21	30	8	30	1
Apr.		Aug.		Dec	
3	6	4	16		
8	15	10	1	6	
14	0	15	10	11	1
19	9	20	19	16	2
24	17	26	4	22	
30	2	31	12	27	1

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1897, inferred	16 46 „	0·1835	67 13

The horizontal force is given in C. G. S. measure.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following star's name are the Nos. in Catalogue, pp. 24–26.)

U Cephei 10 *.

$(2^{d}\ 11^{h}\ 49\frac{1}{2}^{m}.)$

Month	d	h	m
Jan.	3	0	35
	8	0	14
	12	23	53
	17	23	33
	22	23	12
	27	22	51
Feb.	1	22	30
	6	22	9
	11	21	48
	16	21	28
	21	21	7
	26	20	46
March.	3	20	25
	8	20	4
	13	19	43
	18	19	22
	23	19	2
	28	18	41
April.	2	18	20
	7	17	59
	12	17	38
	17	17	17
	22	16	56
	27	16	36
May.	2	16	15
	7	15	54
	12	15	33
	17	15	12
	22	14	51
	27	14	30
June.	1	14	10
	6	13	49
	11	13	28
	16	13	17
	21	12	46
	26	12	25
July.	1	12	4
	6	11	44
	11	11	23
	16	11	2
	21	10	41
	26	10	20
	31	9	59
August.	5	9	38
	10	9	17
	15	8	57
	20	8	36
	25	8	15
	30	7	54
Sept.	4	7	33
	9	7	12
	14	6	51
	19	6	30
	24	6	10
	29	5	49
Oct.	4	5	28
	9	5	7
	14	4	46
	19	4	25
	24	4	4
	29	3	43
Nov.	3	3	22
	8	3	2
	13	2	41
	18	2	20
	23	1	59
	28	1	38
Dec.	3	1	17
	8	0	56
	13	0	35
	18	0	14
	22	23	54
	27	23	33

Algol 27 *.

$(2^{d}\ 20^{h}\ 49^{m}.)$

Month	d	h	m
Jan.	1	3	24
	6	21	2
	12	14	40
	18	8	18
	24	1	56
	29	19	33
Feb.	4	13	11
	10	6	49
	16	0	27
	21	18	5
	27	11	42
March.	5	5	20
	10	22	58
	16	16	36
	22	10	14
	28	3	51
April.	2	21	29
	8	15	7
	14	8	46
	20	2	22
	25	20	0
June.	25	1	7
	30	18	45
July.	6	12	23
	12	6	0
	17	23	38
	23	17	16
	29	10	54
August.	4	4	32
	9	22	9
	15	15	47
	21	9	25
	27	3	3
Sept.	1	20	41
	7	14	18
	13	7	56
	19	1	34
	24	19	12
	30	12	50
Oct.	6	6	27
	12	0	5
	17	17	43
	23	11	21
	29	4	58
Nov.	3	22	36
	9	16	14
	15	9	47
	21	3	30
	26	21	7
Dec.	2	14	45
	8	8	23
	14	2	1
	19	19	39
	25	13	16
	31	6	54

λ Tauri 31 *.

$(3^{d}\ 22^{h}\ 52^{m}.)$

Month	d	h	m
Jan.	3	23	53
	11	21	38
	19	19	22
	27	17	6
Feb.	4	14	51
	12	12	35
	20	10	20
	28	8	4
March.	8	5	48
	16	3	33
	24	1	17
	31	23	2
July.	4	19	54
	12	17	39
	20	15	23
	28	13	8

λ Tauri 31 *

(*continued*).

Month	d	h	m
August.	5	10	52
	13	8	36
	21	6	21
	29	4	5
Sept.	6	1	50
	13	23	34
	21	21	18
	29	19	3
Oct.	7	16	47
	15	14	32
Oct.	23	12	16
	31	10	0
Nov.	8	7	45
	16	5	29
	24	3	14
Dec.	2	0	58
	9	22	42
	17	20	27
	25	18	11

R Canis Maj. 61 †.

$\left\{\begin{matrix} 1 = 1^{d}\ 3^{h}\ 16^{m}. \\ 2 = 2\ \ 6\ \ 31 \end{matrix}\right\}$

Month	d	h	m
Jan.	1	18	57
	5	4	45
	8	14	32
	12	0	19
	15	10	6
	18	19	54
	22	5	41
	25	15	28
	29	1	16
Feb.	1	11	3
	4	20	50
	8	6	38
	11	16	25
	15	2	12
	18	11	59
	21	21	47
	25	7	34
	28	17	21
March.	4	3	9
	7	12	56
	10	22	43
	14	8	31
March.	17	18	18
	21	4	5
	24	13	52
	27	23	40
	31	9	27
April.	3	19	14
	7	5	2
	10	14	49
	14	0	36
	17	10	24
	20	20	11
	24	5	58
	27	15	45
May.	1	1	33
	4	11	20
	7	21	7
	11	6	57
	14	16	42
	18	2	29
	21	12	17
	24	22	4
	28	7	51

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 61†
(*continued*).

Sept.

d	h	m
4	3	43
7	13	30
10	23	18
14	9	5
17	18	52
21	4	39
24	14	27
28	0	14

Oct.

d	h	m
1	10	1
4	19	49
8	5	36
11	15	23
15	1	10
18	10	58
21	20	47
25	6	32
28	16	20

Nov.

d	h	m
1	2	7
4	11	54
7	21	42
11	7	29
14	17	16
18	3	3
21	12	51
24	22	38
28	8	25

Dec.

d	h	m
1	18	13
5	4	0
8	13	47
11	23	35
15	9	22
18	19	9
22	4	56
25	14	44
29	0	31

S Cancri 76.

Jan.

d	h	m
3	3	12
12	14	39
22	2	16
31	13	54

Feb.

d	h	m
10	1	32
19	13	10

March.

d	h	m
1	0	47
10	12	25
20	0	3
29	11	41

April.

d	h	m
7	23	18
17	10	56
26	22	34

May.

d	h	m
6	10	12
15	21	49
25	9	27

June.

d	h	m
3	21	5

June.

d	h	m
13	8	43
22	20	20

July.

d	h	m
2	7	58
11	19	36
21	7	14
30	18	51

Sept.

d	h	m
6	17	22
16	5	0
25	16	38

Oct.

d	h	m
5	4	16
14	15	53
24	3	31

Nov.

d	h	m
2	15	9
12	2	49
21	14	24

Dec.

d	h	m
1	2	2
10	13	40
20	1	18
29	12	55

δ Libræ 132*.
(2^{d} 7^{h} 51^{m}.)

Jan.

d	h	m
1	23	10
6	14	52
11	6	35
15	22	18
20	14	1
25	5	43
29	21	26

Feb.

d	h	m
3	13	9
8	4	52
12	20	34
17	12	17
22	4	0
26	19	43

March.

d	h	m
3	11	26
8	3	8
12	18	51
17	10	34
22	2	17
26	17	59
31	9	42

April.

d	h	m
5	1	25
9	17	8
14	8	50
19	0	33
23	16	16
28	7	59

May

d	h	m
2	23	40
7	15	24
12	7	7
16	22	50
21	14	32
26	6	15
30	21	58

June.

d	h	m
4	13	41
9	5	23
13	21	6
18	12	49
23	4	32
27	20	15

July.

d	h	m
2	11	57
7	3	40
11	19	23
16	11	6
21	2	48
25	18	31
30	10	14

August.

d	h	m
4	1	57
8	17	39
13	9	22
18	1	5
22	16	48
27	8	30

Sept.

d	h	m
1	0	13
5	15	56
10	7	39
14	23	21
19	15	4
24	6	47
28	22	30

Dec.

d	h	m
5	10	20
10	2	2
14	17	47
19	9	28
24	1	11
28	16	54

U Coronæ 137*
(3^{d} 10^{h} 51^{m}.)

Jan.

d	h	m
2	10	27
9	8	9
16	5	51
23	3	33
30	1	15

Feb.

d	h	m
5	22	57
12	20	39
19	18	21
26	16	3

U Coronæ 137*
(*continued*).

March.

d	h	m
5	13	47
12	11	27
19	9	9
26	6	51

April.

d	h	m
2	4	33
9	2	16
15	23	58
22	21	40
29	19	22

May.

d	h	m
6	17	4
13	14	44
20	12	28
27	10	10

June.

d	h	m
3	7	52
10	5	34
17	3	16
24	0	58
30	22	40

July.

d	h	m
7	20	22
14	18	5
21	15	47
28	13	29

August.

d	h	m
4	11	11
11	8	53
18	6	35
25	4	17

Sept.

d	h	m
1	1	59
7	23	41
14	21	23
21	19	5
28	16	47

Oct.

d	h	m
5	14	29
12	12	11
19	9	53
26	7	35

Nov.

d	h	m
2	5	18
9	3	0
16	0	42
22	22	24
29	20	6

Dec.

d	h	m
6	17	48
13	15	30
20	13	12
27	10	54

U Ophiuchi 174‡.

$$\left\{\begin{array}{llll} 1 = 0^{d} & 20^{h} & 8^{m} \\ 2 = 1 & 16 & 16 \\ 3 = 2 & 12 & 23 \end{array}\right\}$$

Jan.

d	h	m
1	10	38
4	19	9
8	3	40
11	12	11
14	20	41
18	5	12
21	13	43
24	22	14
28	6	45
31	15	16

Feb.

d	h	m
3	23	47
7	8	18
10	16	48
14	1	19

Feb.

d	h	m
17	9	50
20	18	21
24	2	52
27	11	23

March.

d	h	m
2	19	54
6	4	25
9	12	56
12	21	26
16	5	57
19	14	28
22	22	59
26	7	30
29	16	1

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

third minimum only.

fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

U Ophiuchi 174 ‡ (*continued*).

d	h	m
April.		
2	0	32
5	9	3
8	17	34
12	2	4
15	10	35
18	19	6
22	3	37
25	12	8
28	20	39
May.		
2	5	10
5	13	41
8	22	12
12	6	42
15	15	15
18	23	44
22	8	15
25	16	46
29	1	17
June.		
1	9	48
4	18	19
8	2	50
11	11	20
14	19	51
18	4	22
21	12	53
24	21	24
28	5	55
July.		
1	14	26
4	22	57
8	7	28
11	15	58
15	0	29

d	h	m
July.		
18	9	0
21	17	31
25	2	2
28	10	33
31	19	4
August.		
4	3	35
7	12	6
10	20	39
14	5	7
17	13	38
20	22	9
24	6	40
27	15	11
30	23	42
Sept.		
3	8	13
6	16	44
10	1	15
13	9	45
16	18	16
20	2	47
23	11	18
26	19	49
30	4	20
Oct.		
3	12	51
6	21	22
10	5	53
13	14	24
16	22	54
20	7	25
23	15	56
27	0	27
30	8	58

Z Herculis 179 ‡.

$$\left\{\begin{array}{lll} 1=2^{d} & 3^{h} & 56^{m} \\ 2=3 & 23 & 49 \\ 3=6 & 3 & 46 \end{array}\right\}$$

d	h	m
Jan.		
1	16	21
9	16	0
17	15	39
25	15	18

d	h	m
Feb.		
2	14	57
10	14	36
18	14	15
26	13	54

Z Herculis 179 ‡ (*continued*).

d	h	m
March.		
6	13	33
14	13	2
22	12	51
30	12	31
April.		
7	12	10
15	11	49
23	11	28
May.		
1	11	7
9	10	46
17	10	25
25	10	4
June.		
2	9	43
10	9	22
18	9	1
26	8	41
July.		
4	8	20
12	7	59

d	h	m
July.		
20	7	38
28	7	17
August.		
5	6	56
13	6	35
21	6	14
29	5	53
Sept.		
6	5	32
14	5	11
22	4	51
30	4	30
Oct.		
8	4	9
16	3	48
24	3	27
Nov.		
1	3	6
9	2	45
17	2	24
25	2	3

W Delphini 221 †.

$$\left\{\begin{array}{lll} 1=4^{d} & 19^{h} & 21^{m} \\ 2=9 & 14 & 42 \end{array}\right\}$$

d	h	m
Jan.		
4	20	34
19	6	37
Feb.		
2	16	41
17	2	44
March.		
3	12	48
17	22	52
April.		
1	8	55
15	18	59
30	5	2

d	h	m
May.		
14	15	6
29	1	10
June.		
12	11	13
26	21	17
July.		
11	7	20
25	17	24
Aug.		
9	3	28
23	13	31

W Delphini 221 † (*continued*).

d	h	m
Sept.		
6	23	35
21	9	38
Oct.		
5	19	42
20	5	46

d	h	m
Nov.		
3	15	49
18	1	53
Dec.		
2	11	56
16	22	0
31	8	4

Y Cygni 235 ‡.

$$\left\{\begin{array}{lll} 1=1^{d} & 2^{h} & 21^{m} \\ 2=2 & 23 & 55 \\ 3=4 & 2 & 16 \end{array}\right\}$$

d	h	m
Jan.		
2	3	18
8	3	7
14	2	57
20	2	47
26	2	35
Feb.		
1	2	27
7	2	17
13	2	6
19	1	56
25	1	46
March.		
3	1	36
9	1	26
15	1	16
21	1	5
27	0	55
April.		
2	0	45
8	0	35
14	0	25
20	0	15
26	0	5
May.		
1	23	54
7	23	44
13	23	34
19	23	24
25	23	14
31	23	4

d	h	m
June.		
6	22	53
12	22	43
18	22	33
24	22	23
30	22	13
July.		
6	22	3
12	21	52
18	21	42
24	21	32
30	21	22
August.		
5	21	12
11	21	2
17	20	51
23	20	41
29	20	31
Sept.		
4	20	21
10	20	11
16	20	1
22	19	50
28	19	40
Oct.		
4	19	30
10	19	20
16	19	8
22	19	0
28	18	50

† Every third minimum only.
‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

Y Cygni 235 ‡ (*continued*).

Nov. d	h	m	Dec. d	h	m
3	18	39	3	17	49
9	18	29	9	17	38
15	18	19	15	17	28
21	18	9	21	17	18
27	17	59	27	17	8

S Antliæ 82 §.

Month	d	h	m
Jan.	1	3	21
	4	9	9
	7	14	57
	10	20	45
	14	2	33
	17	8	21
	20	14	9
	23	19	57
	27	1	45
	30	7	33
Feb.	2	13	21
	5	19	9
	9	0	57
	12	6	45
	15	12	33
	18	18	21
	22	0	9
	25	5	57
	28	11	45
March.	3	17	33
	6	23	21
	10	5	9
	13	10	57
	16	16	45

S Antliæ 82 § (*continued*).

Month	d	h	m
March.	19	22	33
	23	4	21
	26	10	11
	29	15	57
April.	1	21	45
	5	3	33
	8	9	21
	11	15	9
	14	20	57
	18	2	45
	21	8	33
	24	14	21
	27	20	9
May.	1	1	57
	4	7	45
	7	13	33
	10	19	21
	14	1	9
	17	6	57
May.	20	12	45
	23	18	33
	27	0	21
	30	6	9
June.	2	11	57
	5	17	45
	8	23	33
	12	5	21
	15	11	9
	18	6	57
	21	22	45
	25	4	33
	28	10	21
July.	1	16	9
	4	21	57
	8	3	45
	11	9	33
	14	15	21
	17	21	9

S Antliæ 82 § (*continued*).

Month	d	h	m
July.	21	2	57
	24	8	45
	27	14	33
Sept.	4	12	9
	7	17	57
	10	23	45
	14	5	33
	17	11	21
	20	17	9
	23	22	57
	27	4	45
	30	10	33
Oct.	3	16	21
	6	22	9
	10	3	57
	13	9	45
	16	15	33
	19	21	21
	23	3	9
Oct.	26	8	57
	29	14	45
Nov.	1	20	33
	5	2	21
	8	8	9
	11	13	57
	14	19	45
	18	1	33
	21	7	21
	24	13	9
	27	18	57
Dec.	1	0	45
	4	6	33
	7	12	21
	10	18	9
	13	23	57
	17	5	45
	20	11	33
	23	17	21
	26	23	9
	30	4	57

‡ Every fourth minimum only.

§ Every tenth minimum only; the others may be found by adding multiples of the period $7^{h}\ 46^{m}{\cdot}8$.

MAXIMA OF U PEGASI.

Month	d	h	m
Jan.	1	3	22
	4	14	26
	8	1	30
	11	12	34
	14	23	37
	18	10	41
	21	21	45
	25	8	49
	28	19	52
Feb.	1	6	56
	4	18	0
	8	5	4
	11	16	7
	15	3	11
	18	14	15
	22	1	19
	25	12	22
	28	23	26
April.	11	12	11
	14	23	15
	18	10	19
	21	21	22
	25	8	26
	28	19	30
May.	2	6	34
	5	17	37
	9	4	41
	12	15	45
	16	2	49
	19	13	52
	23	0	56
	26	12	0
	29	23	4
June.	2	10	7
	5	21	11
	9	8	15
	12	19	19
	16	6	22
	19	17	26
	23	4	30
	26	15	34
	30	2	37
July.	3	13	41
	7	0	45
	10	11	49
	13	22	52
	17	9	56
	20	21	0
	24	8	4
	27	19	7
	31	6	11
August.	3	17	15
	7	4	19
	10	15	22
	14	2	26
	17	13	30
	21	0	36
	24	11	37
	27	22	41
	31	9	45
Sept.	3	20	49
	7	7	52
	10	18	56
	14	6	0
	17	17	4
	21	4	7
	24	15	11
	28	2	15
Oct.	1	13	19
	5	0	22
	8	11	26
	11	22	30
	15	9	34
	18	20	37
	22	7	41
	25	18	45
	29	5	49
Nov.	1	16	52
	5	3	56
	8	15	0
	12	2	4
	15	13	7
	19	0	11
	22	11	15
	25	22	19
	29	9	22
Dec.	2	20	26
	6	7	30
	9	18	34
	13	5	37
	16	16	41
	20	3	45
	23	14	49
	27	1	52
	30	12	56

Every fifteenth maximum is given; the others may be found by adding *multiples of the period* $5^{h}\ 32^{m}{\cdot}3$.

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to Catalogue on pp. 24–26; M signifies maximum; *m*, minimum.

January.

d	No.
1	261 M
	155 M
	170 M
	13 M
	69 M
2	114 *m*
	222 M
	212 *m*
5	127 *m*
	144 M
6	229 M
10	59 M
	39 *m*
12	33 M
	96 *m*
13	16 M
	157 M
14	198 M
15	108 M
	14 M
16	35 M
	89 *m*
	258 M
17	6 *m*
	63 M
18	5 M
19	68 M
21	118 *m*
	190 M
22	36 M
23	86 M
25	264 M
	194 M
	116 *m*
26	236 *m*
	19 *m*
	197 *m*
27	58 *m*
	138 M
28	204 M
	240 M
29	255 M
	233 M
31	217 *m*
	181 M
	109 M
	89 M

February.

d	No.
1	29 *m*
	205 *m*
2	42 M
	259 M
	152 M
3	145 M
	156 M
6	3 M
7	92 M
8	160 *m*
9	166 M
10	129 M
11	105 *m*
14	63 *m*
	190 *m*
	62 *m*
	131 M
15	162 M
16	217 M

February (*con.*).

d	No.
16	168 *m*
17	227 M
	60 M
18	192 M
	143 *m*
	87 *m*
20	97 M
	89 *m*
21	242 M
23	207 M
24	21 *m*
25	238 *m*
26	72 *m*
	200 *m*
	175 M
	121 *m*
27	37 *m*
	112 M
28	205 M

March.

d	No.
2	7 *m*
	102 M
3	171 *m*
	250 M
4	63 M
6	152 *m*
7	164 M
	169 M
	120 M
	89 M
8	116 M
9	91 M
	190 M
	119 M
12	215 M
13	140 M
14	224 M
15	106 *m*
	56 *m*
16	1 M
	142 M
18	222 *m*
	49 M
	22 M
21	135 M
23	134 M
	45 M
	124 M
24	78 M
	260 *m*
25	47 *m*
26	67 M
	48 M
27	51 M
	89 *m*
28	90 M
29	236 M
	69 M
	24 M
30	9 M

April.

d	No.
1	63 *m*
	130 M
2	190 *m*

April (*con.*).

d	No.
2	8 *m*
3	257 *m*
	46 *m*
6	232 *m*
10	243 M
	205 *m*
11	217 *m*
	89 M
12	127 M
	218 M
	252 M
14	199 M
	55 M
15	14 *m*
	209 M
	125 M
	244 M
	219 *m*
17	200 M
	38 *m*
18	64 *m*
19	63 M
20	113 M
	128 *m*
	156 *m*
22	172 M
	216 *m*
23	148 *m*
	242 *m*
24	190 M
27	116 *m*
	217 M
	181 *m*
	121 M
28	19 M
	23 M
29	60 *m*
	203 M
	138 *m*

May.

d	No.
1	104 *m*
	89 *m*
3	213 M
4	108 *m*
5	21 M
	152 M
6	237 M
	205 M
7	212 M
	41 *m*
	131 *m*
	18 *m*
	147 M
8	29 M
	161 *m*
9	143 M
	12 *m*
10	91 *m*
11	226 M
12	204 *m*
14	183 M
	198 *m*
15	239 *m*
	11 M
	126 *m*
16	89 M

May (*con.*).

d	No.
17	96 M
	63 *m*
	88 *m*
18	141 M
	190 *m*
20	258 *m*
	4 *m*
	37 M
21	230 M
	222 M
	261 *m*
22	106 M
23	247 M
25	215 *m*
26	98 M
28	58 M
	158 M
	240 *m*
29	105 M
30	109 *m*

June.

d	No.
4	63 M
5	25 *m*
	186 M
	89 *m*
6	206 M
7	152 *m*
8	114 M
	116 M
	168 M
10	139 M
	190 M
11	248 M
12	2 *m*
	236 *m*
14	251 M
	220 *m*
15	72 M
16	205 *m*
17	86 *m*
	30 M
20	223 M
	217 *m*
	197 M
	89 M
22	238 M
23	69 M
24	43 *m*
	256 M
25	263 *m*
26	62 M
27	246 M
28	85 *m*

July.

d	No.
2	63 *m*
	242 M
3	232 M
	190 *m*
4	202 *m*
6	6 M
	171 M
	217 M
7	14 M
	60 M
9	262 M

July (*con.*).

d	No.
10	195 M
	241 M
	22 *m*
	50 *m*
	44 M
11	89 *m*
	8 M
13	205 M
14	181 M
15	173 M
16	33 *m*
	118 M
18	216 M
19	155 M
	166 *m*
20	20 *m*
	245 M
	63 M
21	74 M
22	99 M
24	96 *m*
25	7 M
	13 *m*
	104 M
	101 M
26	89 M
	16 *m*
	115 M
	190 M
	259 *m*
27	116 *m*
	70 M
	117 M
28	229 M
30	35 *m*
	144 M
31	128 M
	19 *m*

August.

d	No.
1	143 *m*
	249 M
3	124 *m*
	134 *m*
	160 M
4	222 *m*
	91 M
5	65 M
	150 M
	56 M
	215 M
	71 *m*
	59 *m*
6	121 *m*
	152 M
7	106 *m*
	87 M
	138 M
8	204 M
	66 *m*
10	200 *m*
	21 *m*
	252 *m*
	131 M
	39 M
13	97 *m*
	236 M

August (*con.*).

d	No.
14	169 *m*
15	89 *m*
	192 *m*
	196 M
16	37 *m*
17	63 *m*
19	190 *m*
20	218 M
21	224 *m*
23	228 M
	205 *m*
	92 *m*
25	108 M
	157 M
26	109 M
28	175 *m*
	52 M
29	29 *m*
	80 *m*
	217 *m*
30	89 M

September.

d	No.
1	198 M
2	258 M
	242 *m*
4	153 M
	41 M
	63 M
6	227 *m*
	48 M
7	116 M
	152 *m*
8	5 M
9	67 *m*
10	190 M
	58a M
11	79 M
12	126 M
	219 M
13	156 M
14	211 M
	217 M
16	60 *m*
17	145 M
	69 M
18	127 *m*
	205 M
19	89 *m*
	46 M
	260 M
21	148 M
	146 M
22	257 M
	77 M
23	214 M
24	23 *m*
	47 M
	212 *m*
27	238 *m*
29	64 M
	200 M
30	172 *m*

October.

d	No.
1	24 *m*
2	63 *m*
4	89 M

October (*con.*).

d	No.
4	51 *m*
	190 *m*
5	14 *m*
	121 M
6	91 *m*
7	222 M
8	181 *m*
	154 M
	9 *m*
	88 M
10	193 M
11	237 M
12	119 M
14	106 M
15	207 *m*
	26 M
17	17 M
19	215 *m*
	21 M
	18 M
	168 *m*
20	143 M
	213 *m*
	63 M
22	161 M
	28 M
23	105 *m*
24	89 *m*
	240 M
25	197 *m*
	40 M
26	116 *m*
	164 M
27	190 M
	163 M
28	232 *m*
	236 *m*
	45 *m*
29	36 M
	3 M
	205 *m*
30	209 *m*
31	19 M
	131 *m*

November.

d	No.
2	230 M
3	68 M
	49 *m*
4	170 M
5	112 M
6	37 M
	159 M
	152 M
7	138 *m*
8	89 M
	217 *m*
9	20 M
	22 M
	147 *m*
10	2 M
11	85 M
	242 M
12	140 *m*
15	233 M
	86 M
	186 *m*
	216 *m*

November (*con.*).

d	No.
16	134 M
17	38 M
	62 *m*
	63 *m*
19	204 *m*
20	190 *m*
21	158 M
	166 M
23	243 *m*
24	217 M
	60 M
	255 M
25	72 *m*
	205 M
26	96 M
28	184 M
	89 *m*
29	156 *m*
30	176 M
	25 M

December.

d	No.
1	230 M
	239 M
	252 M
	128 *m*
3	29 M
	33 M
5	63 M
6	78 M
	104 *m*
	113 *m*
7	124 M
	116 M
	160 *m*
8	92 M
	71 M
9	152 *m*
10	171 *m*
11	108 *m*
12	190 M
13	69 M
	89 M
	59 M
16	257 M
17	224 M
18	135 M
20	222 *m*
22	16 M
23	109 *m*
24	127 M
25	181 M
	11a M
	118 *m*
	141 M
27	14 M
	90 M
	261 M
28	203 M
	218 M
29	236 M
	106 *m*
30	215 M
	263 M
31	198 *m*
	97 M
	91 M

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1890+.	Observer.
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½; 7½	332·0	1·64	5·86	C.
2.	Σ 13	0 10·3	+76 24	6½; 7	87·7	0·92	5·90	C.
3.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½; 6½	148·6	0·58	4·03	B.
4.	OΣ 18	0 37·2	+ 3 36	7½; 9½	127·2	1·47	4·83	B.
5.	β 866	0 39·1	+42 34	9 ; 9	69·6	1·39	1·83	β.
6.	Σ 60, η Cassiop.	0 43·0	+57 17	3½; 7½	202·9	4·65	5·72	M.
7.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	335·0	0·32	5·90	Ls.
8.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	11·2	1·18	5·90	Ls.
9.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½; 7	349·7	1·68	4·88	Ls.
10.	Σ 138	1 30·8	+ 7 7	7 ; 7½	37·5	1·51	5·92	Ls.
11.	*p* Eridani	1 35·7	−56 0	6 ; 6	224·0	7·35	4·96	S.
12.	β 509	1 38·2	+ 8 53	8 ; 8½	257·5	0·74	5·91	Ls.
13.	Σ 202, α Piscium	1 56·8	+ 2 17	4 ; 5	320·2	2·78	5·85	C.
14.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½; 5½	63·2	10·33	5·88	Ls.
15.	Σ 228	2 7·3	+56 59	6½; 7½	66·9	0·27	6·09	Ls.
16.	Σ 305	2 41·8	+18 58	7½; 8	315·9	2·90	6·08	Ls.
17.	Σ 333, ε Arietis	2 53·5	+20 56	5½; 6	202·1	1·34	6·08	Ls.
18.	OΣ 50	3 1·5	+71 4	7 ; 7	205·9	1·27	3·21	M.
19.	β 530	3 9·1	+22 33	9½; 10	182·7	1·68	0·10	Ls.
20.	Σ 412, 7 Tauri	3 28·5	+24 8	6½; 7	204·5	0·30	6·08	Ls.
21.	Σ 422	3 31·8	+ 0 16	6 ; 8	247·1	6·19	5·90	C.
22.	Σ 425	3 33·8	+23 42	7½; 8	92·7	3·00	4·19	M.
23.	Σ 460	3 53·2	+80 25	6 ; 7	43·8	0·96	3·21	M.
24.	OΣ 531	4 0·5	+37 48	7 ; 7½	127·3	2·45	5·88	Ls.
25.	Σ 518, o^2 Eridani	4 10·6	− 7 49	8½; 10½	87·4	2·32	5·91	C.
26.	OΣ 79, 55 Tauri	4 14·2	+16 17	6½; 8	87·9	0·38	0·67	β.
27.	Σ 535	4 17·7	+11 9	7 ; 8	330·5	2·07	5·06	Ls.
28.	Σ 572	4 32·2	+26 44	6 ; 6	202·1	3·69	4·13	M.
29.	OΣ 98, ι Orionis	5 2·4	+ 8 22	5½; 7	181·7	1·01	6·02	M.
30.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	258·8	3·50	5·16	M.
31.	Σ 749	5 30·9	+26 53	6½; 7½	177·3	0·86	6·11	Ls.
32.	Σ 932	6 28·5	+14 49	8 ; 8	328·1	2·34	3·18	Ls.
33.	Σ 945	6 33·3	+41 4	7 ; 8	269·0	0·54	3·24	Ls.
34.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	120·2	1·80	6·07	C.
35.	Sirius	6 40·6	−16 34	1 ; 10	189·6	3·65	6·82	Sch.
36.	Σ 982, 38 Geminor.	6 49·0	+13 18	5½; 8	160·7	6·77	3·21	Ls.
37.	Σ 1037	7 6·6	+27 24	7 ; 7	307·0	1·01	6·08	C.
38.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	112·4	0·74	3·15	Ls.
39.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	205·3	6·89	6·08	C.
40.	Σ 1074	7 15·4	+ 0 36	7½; 8½	133·8	0·68	6·18	Ls.
41.	OΣ 175	7 27·7	+31 10	6 ; 6½	324·2	1·02	6·08	Ls.
42.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	227·4	5·70	6·07	Ls.
43.	OΣ 182	7 47·4	+ 3 39	8½; 8½	31·6	0·94	6·27	Ls.
44.	Σ 1187	8 3·2	+32 31	7 ; 8	46·0	2·17	6·15	Ls.
45.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½; 7½	18·1	1·03	6·18	Ls.
46.	„ „ AC	……	……	5½; 6½	116·3	5·22	6·18	Ls.
47.	„ „ BC	……	……	7½; 6½	128·9	5·50	6·18	Ls.
48.	β 205	8 28·8	−24 16	7 ; 7	252·6	0·3	3·20	S.
49.	Σ 1263	8 38·6	+42 4	7½; 8	20·6	51·42	5·30	Ls.
50.	Σ 1273, ε Hydræ	8 41·5	+ 6 50	4 ; 7	231·4	3·68	5·23	Ls.
51.	Σ 3121	9 11·9	+29 0	7½; 7½	194·5	0·68	6·15	Ls.
52.	Σ 1348	9 19·2	+ 6 50	7½; 7½	322·0	1·77	6·20	Ls.
53.	OΣ 201	9 18·0	+28 20	7 ; 11	221·6	1·34	6·22	Ls.
54.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½; 7	108·0	0·56	6·19	Ls.
55.	Σ 1389	9 45·3	+27 29	8 ; 9	309·5	2·48	6·20	Ls.
56.	Σ 1417	10 9·5	+19 40	8½; 8½	257·7	2·56	2·29	M.
57.	OΣ 215, P. x. 23	10 10·9	+18 15	6½; 7	209·2	0·82	6·24	Ls.
58.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	115·6	3·63	6·31	Ls.
59.	OΣ 218	10 22·1	+ 4 5	7 ; 9	74·0	1·04	6·30	Ls.
60.	OΣ 224	10 34·5	+ 9 22	7 ; 9	313·4	0·40	6·26	Ls.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1890+.	Observer.
		h m	° ′		°	″		
61.	OΣ 229	10 42·3	+41 38	6 ; 7	322·9	0·83	2·44	M.
62.	Σ 1523, ξ Ursæ Maj. .	11 12·9	+32 6	4 ; 5	170·6	2·00	6·31	Ls.
63.	Σ 1536, ι Leonis	11 18·7	+11 5	4½ ; 7½	59·6	2·53	6·34	Ls.
64.	OΣ 235	11 26·7	+61 38	5½ ; 7	84·3	0·97	2·12	β.
65.	Σ 1606	12 5·7	+40 27	6 ; 7	330·2	1·10	5·57	M.
66.	Σ 1647, 191 Virginis...	12 25·5	+10 16	7½ ; 8	219·6	1·32	3·36	Ls.
67.	Σ 1670, γ Virginis ...	12 36·7	− 0 54	3 ; 3	153·2	5·96	6·36	Ls.
68.	Σ 1687, 35 Comæ......	12 48·1	+21 48	5 ; 8	74·8	1·11	6·34	Ls.
69.	β 112	12 55·2	+19 1	9 ; 9½	292·0	2·24	6·41	Ls.
70.	OΣ 266	13 23·6	+16 14	7 ; 8	339·1	1·70	6·41	Ls.
71.	Σ 1757, P. xiii. 127 ...	13 29·3	+ 0 12	7½ ; 8	80·0	2·62	3·42	Ls.
72.	Σ 1768, 25 Can. Ven. .	13 33·2	+36 48	6 ; 8	144·1	0·93	5·51	M.
73.	Σ 1785	13 44·5	+27 29	7½ ; 8	263·2	1·44	6·38	Ls.
74.	Σ 1788	13 49·7	− 7 34	6½ ; 7½	75·5	2·60	6·38	M.
75.	Σ 1819	14 10·3	+ 3 36	7½ ; 7½	185·4	1·38	4·52	M.
76.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	273·2	0·20	6·38	Ls.
77.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	326·1	2·70	6·46	M.
78.	Σ 1879	14 41·3	+10 4	8 ; 8½	144·8	0·50	6·38	Ls.
79.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	220·6	2·83	6·38	Ls.
80.	β 31	14 47·8	+19 9	8½ ; 9½	194·5	1·55	6·37	Ls.
81.	OΣ 287	14 47·9	+45 20	7 ; 8	316·3	0·85	6·68	M.
82.	β 119	15 0·0	− 6 36	8 ; 8½	307·1	1·47	2·35	L.
83.	β 350	15 8·2	−27 10	7 ; 8½	160·2	1·15	2·41	L.
84.	Σ 1932, 1 Cor. Bor....	15 14·0	+27 13	6½ ; 7	321·8	0·74	6·36	Ls.
85.	Σ 1937, η Cor. Bor....	15 19·1	+30 40	6 ; 6	310·8	0·41	6·38	Ls.
86.	Σ 1938, μ^2 Boötis......	15 20·7	+37 22	6½ ; 7½	82·2	0·92	6·56	M.
87.	OΣ 298	15 32·4	+40 8	7 ; 7½	170·6	1·03	3·46	Ls.
88.	Σ 1967, γ Cor. Bor....	15 38·5	+26 36	4 ; 7	118·5	0·60	6·45	Ls.
89.	OΣ 303	15 56·2	+13 33	7 ; 7½	138·4	0·89	6·35	Ls.
90.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	215·3	0·94	6·46	M.
91.	Σ 2032, σ Coronæ......	16 11·1	+34 7	6 ; 6½	210·6	4·16	6·45	Ls.
92.	Σ 2055, λ Ophiuchi ...	16 25·8	+ 2 12	4½ ; 5½	47·0	1·40	6·44	Ls.
93.	Σ 2084, ζ Herculis ...	16 37·5	+31 47	3 ; 6½	6·0	0·53	6·44	Ls.
94.	Σ 2107, 167 Herculis .	16 47·8	+28 50	7 ; 8½	292·2	0·51	6·48	Ls.
95.	Σ 2140, α Herculis ...	17 10·1	+14 30	2½ ; 6	115·8	4·93	6·52	Ls.
96.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	336·4	0·97	6·54	Ls.
97.	OΣ 338	17 47·4	+15 21	6½ ; 6½	14·4	0·72	6·50	Ls.
98.	Σ 2262, τ Ophiuchi ...	17 57·6	− 8 11	5 ; 6	253·9	1·84	3·47	M.
99.	Σ 2272, 70 Ophiuchi .	18 0·4	+ 2 32	4 ; 6	290·2	2·08	6·62	Le.
100.	OΣ 358	18 31·3	+16 54	6½ ; 7	195·3	1·98	6·50	Ls.
101.	β 648	18 54·2	+23 39	6 ; 9	233·4	1·26	6·56	Ls.
102.	γ Coronæ Aust..........	19 0·0	−37 12	5½ ; 5½	164·0	1·03	4·71	S.
103.	β 248	19 13·4	+22 51	6 ; 9	125·4	1·86	0·63	β.
104.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	303·8	1·75	6·80	M.
105.	Σ 2695	20 27·7	+25 28	6 ; 8	76·3	1·12	6·62	Ls.
106.	β 151, β Delphini ...	20 32·7	+14 15	3½ ; 5½	338·7	0·20	5·81	Ls.
107.	Σ 2729	20 46·3	− 5 59	6½ ; 6½	191·8	0·20	5·81	Ls.
108.	Σ 2799	21 23·9	+10 38	7 ; 7½	119·4	1·22	5·72	Ls.
109.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	123·2	2·91	5·75	Ls.
110.	Σ 2909, ζ Aquarii......	22 23·7	− 0 32	4 ; 4	322·7	3·31	6·86	M.
111.	Σ 2944, AB	22 42·7	− 4 45	7 ; 7½	255·5	3·50	2·76	M.
112.	OΣ 483, 52 Pegasi......	22 54·2	+11 12	6 ; 7½	216·9	1·10	1·29	M.
113.	β 79	23 11·8	− 2 9	8 ; 8½	94·6	0·88	1·54	β.
114.	β 80	23 13·2	+ 4 53	8 ; 8½	325·1	0·64	4·85	Ls.
115.	OΣ 547	0 0·4	+45 16	8 ; 8	340·8	4·30	1·73	H.

In the column headed "Observer," the following abbreviations are used:—

L.	Leavenworth.	β.	Burnham.	M.	Maw.	S.	Sellors.
Ls.	Lewis.	B.	Bijourdan.	H.	Hall.	C.	Collins.
Sch.	Schaeberle.						

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 249. JANUARY. 1897.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, December 11, 1896.

Dr. A. A. Common, F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., and
E. W. Maunder.

The Minutes of the last Meeting were read and confirmed.

Mr. Maunder. Since the date of the last Meeting sixty-four presents have been received by the Society. Among these are two books, biographies of two past Presidents of the Society now deceased, Prof. Charles Pritchard and Sir G. B. Airy. The former is presented by Miss Pritchard, the latter by Mr. W. T. Lynn. There is also a scrap-book of letters, drawings, &c., relating to the Solar Eclipse of 1860, made by J. Bononi, and a manuscript book of observations of the same eclipse by Prof. Pritchard, both presented by his executors. We have also among these presents a book on Hindoo Astronomy presented by R. Narayan Rai, a memoir containing observations of Mars in 1883 and 1884 by Prof. Schiaparelli, and seven positive enlargements made by Dr. Weinek from lunar negatives taken at the Lick Observatory.

The President. Dr. Weinek's addition to the series of enlargements of photographs of the lunar surface that we already possess are interesting as showing just about the limiting scale to which it is possible to enlarge photographs with advantage. I will ask you to return your thanks to the donors of these presents.

A vote of thanks was accorded unanimously.

On the motion of Mr. W. B. Gibbs, seconded by Mr. Inwards, Mr. D. Smart, Mr. C. Thwaites, and Mr A. C. D. Crommelin were elected Auditors of the accounts of the Society for the past year.

Dr. Johnstone Stoney. The ephemeris of the Leonid stream which I now present to the Society is of that part of the stream through which the Earth passed in 1866, and is intended to enable astronomers to make their first attempt next spring to photograph the meteors while at a distance from the Earth in space outside our atmosphere. Any computation which can be made leads to the conclusion that the meteor-stream is excessively [illegible], so that no one should be discouraged if his efforts on this occasion are unsuccessful. It will then be well to renew the experiment in 1898, when the stream will have come nearer the Sun. Next spring the stream will be drawn out to an apparent length of at least five degrees and possibly to eight degrees, so that if it can be photographed at all it will appear as a stream across the photographic plate and about 20″ wide. The uncertainty of the data from which the ephemeris is computed makes it desirable that a full field on each side of the track indicated by the ephemeris shall be explored. The ephemeris has been computed by Mr. Wright of the 'Nautical Almanac,' under Dr. Downing's direction. It is probable that no observation will be possible next January, owing to the low position of the swarm in the eastern sky and the approach of dawn, nor in the middle of the succeeding months on account of moonlight; but in the earlier and later parts of February, March, and April everything will be favourable and will become more favourable as the season advances, until in April the best conditions will prevail. Soon after that twilight will be troublesome. There may be only a distant prospect of being able to photograph the meteors outside our atmosphere, but it seems almost an astronomical duty to make the attempt, and it will be made under favourable circumstances, for Dr. Isaac Roberts has undertaken to be one of the astronomers who will try this experiment.

Dr. A. A. Rambaut. Although our observations this year in Dublin of the meteors which form part of the stream of Leonids fell far short of what we hoped for, and in point of accuracy and completeness left a great deal to be desired, yet I think, in view of the fact that weather conditions prevented observations at a great many stations, our observations may possibly be of value and ought to be put on record. Cloudy weather prevented observations on the 12th. On the 13th my assistant, Mr. Charles Martin, began his watch at $12^h\ 55^m$ G.M.T. At that time the sky was cloudy, especially in the East, and a thin film of cloud, through which stars could be seen, was spread all over the sky. This, however, began to clear up shortly afterwards, and by $13^h\ 55^m$ had quite disappeared, so that after that time observations were made in a clear sky. The first meteor was recorded at $14^h\ 16^m$, and was followed by others. The last meteor was noticed at $16^h\ 14^m$ G.M.T. Shortly after this clouds began to come up and the sky was completely overcast. At $16^h\ 55^m$ the watch was discontinued. The tracks of these meteors were plotted down by Mr. Martin upon the map of the constellation Leo which had previously been prepared, and the results of his observations are introduced in a diagram, a copy of which is now shown on the screen.

From this it will be seen that they were not numerous. On the following day I attached a camera to our photographic equatorial telescope, in the hope of getting records of trails. The lens was a rapid rectilinear of 1¼ inches aperture and about 8½ inches focal length. The size of the plate was 7½ inches by 5, which would have covered a region of the sky of about 50 degrees by 33, but not a single trace was found upon the plate after development. It will at once be objected that the aperture was too small, but I did not hope to find traces of any but bright meteors; yet I confess I was disappointed that some of the brighter ones did not leave a mark, especially as we saw some crossing the field of view covered by the plate, estimated as of first or second magnitude. This may be of some use in affording a lower limit to the aperture which may be used. If fainter meteor-trails are to be obtained, apertures considerably larger must be used. This experiment impaired the completeness of our watch on the second evening, for I found it necessary to call in Mr. Martin to help me in exposing and covering the lens. The observations which he conducted, as on the previous night, were thus three times interrupted, but the watch continued without interruption for three intervals of an hour at a time. The results are given in a table, and the tracks are shown on the screen. It will be seen that the trails are more divergent from a single point. In analyzing these observations I have counted the number of meteors noticed in each successive period of ten minutes. The number of meteors was never greater than four in ten minutes. At first sight these observations seem to point to an increase in the number of meteors on both nights in the interval between $14^h\ 30^m$ and $16^h\ 30^m$, which, if real, would be somewhat difficult to account for; but I think the apparent falling off in the point of numbers towards the end of the first night's watch was possibly due to the coming-on of clouds which preceded total obscuration, and on the second night the light of the Moon, which set at about $14^h\ 49^m$ towards the beginning of the watch, and the small altitude of the constellation Leo to some extent conspired to render some of the meteors invisible. The increase in the numbers at $14^h\ 40^m$ on November 13, and falling-off at $16^h\ 30^m$ on November 14, is, I think, real, and it seems to me that the deduction from these observations is that the densest part of the stream was encountered in the interval between our two watches, and in fact may have occurred during the daylight hours of the 14th. I understand that there was a numerous flight of meteors seen at one or two stations in America. From the tracks laid down by Mr. Martin it would appear that the meteor-trails of the 14th diverged much more from a point than those of the 13th, although I would not rely too much upon the accuracy of any particular track, as I think that the proximity of a bright star has, perhaps, biassed the estimates of the observer.

Prof. Alex. Herschel. There is a peculiarity of the Leonid meteor-system which may perhaps explain the difficulty about the observed time of appearance of the shower this year. At the last great return of the Leonids it was distinctly shown by

Mr. B. V. Marsh, of Philadelphia [*], that the shower consisted of three separate meteor-streams, one stream on each side of, and about twelve hours distant from, a middle stream. The three streams were nearly equal in brightness, and the side-streams about thrice as wide as the middle one. The central stream's recurrence in England this year, towards daybreak on the 14th of November, was true to its appointed time. It was seen beginning, in Madeira, at just about that time [†], having been insignificant in brightness up to 4.30 there (about 5½ A.M., Greenwich Time), and up to after 4¼ A.M. in England [‡]. But the after-stream was seen more perfectly and brightly on the morning of November 15th, because the Earth was then but just out of the middle part, and was leaving the end portion of the after-stream. The leading, or preceding shower, twelve hours before the middle stream, fell so late in the day of November 13th, as to be quite hidden by daylight from English observations, and Leonids were only noted in very scanty numbers, in England, on the morning of that day, as might be expected from the Earth having probably traversed this first branch-stream in the afternoon and evening part of the 13th. If the appearances of the shower this year, perhaps, agreed thus satisfactorily with its triple structure, then although the conditions of the Earth's passage through the streams next year will probably disappoint the expectations of English watchers for the main stream, by its return on November 14th next entirely in daytime, yet they may be hoped to be very favourable still to observations, for which they will offer this advantage, that the end and beginning parts repectively of the front- and rear-side streams will be visible about equally on the mornings of November 14th and 15th, next year; so that, besides affording a good opportunity, if both nights are fine, for comparing together the intensities of the two displays, expectations of observing the meteors, if either of the nights should happen to be cloudy, may still very possibly be satisfied by fine weather on the other morning. It is a great advantage to look forward to, and I hope we shall be able to compare the strength of those two streams next year, although we may not see the central stream so well.

Professor Turner. Watch was kept for these meteors at Oxford, but with little success, owing to bad weather. I think that any observations of the meteors we get at this return will be valuable, for I looked up the observations of meteors in the Greenwich volume, at the 1866 return, and found that no particular care had been taken to observe meteors at all until 1865, when the British Association circular directed attention to the matter; and as observation of meteors at the Royal Observatory seems to have been begun in that year, for the years corresponding to the present and next years—1863 and 1864—we have no record at the last return. I would like to ask Dr. Johnstone Stoney and others who have time to look up matters of this kind, whether they have paid any attention to the other large stream of November, the stream which gave a fine display in 1872 and 1885, and I suppose will be recurring in 1898. In fact we are nearer to that return than we are to 1899. I do not think the stream is scattered so much along the orbit; but in 1897 it may be well to look out for November 27th meteors as well as November 14th.

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[*] 'Reports of the British Association,' 1869, pp. 302, 303.

[†] Letter from Mr. W. Anderson, in the 'English Mechanic,' Dec. 11, 1896.

[‡] 'English Mechanic,' Nov. 27, 1896.

Dr. Spitta. Can Dr. Rambaut give us any idea of the apparent magnitudes of the meteors? He has given the minimum aperture of the lens to be employed, and I should like to know if he has got any idea of colour, and whether he used any isochromatic plates. It must be a very difficult matter to photograph them because of their rapidity.

The President. These two papers are of very great importance indeed, as they involve a question which I do not think has ever been brought to our notice before, and that is the possibility of photographing these streams as self-luminous objects some distance away from the Earth. Some authorities on meteors and meteoric hypotheses have said that these bodies travel in their orbits without emitting any light, and become self-luminous only when they meet the Earth's atmosphere. Dr. Stoney suggests that they may reflect light, and so be able to be photographed, as the tail of a comet would be at some distance from the Earth. That is an idea that has not been brought forward before, and it is worthy of serious consideration. If these meteors partake of the nature of comets' tails, which we understand they may do, then it is possible that we may be able to photograph a stream of meteors, which is entirely and distinctly separate from the possibility of photographing a meteor when it has struck the Earth's atmosphere and becomes luminous.

Mr. F. C. Penrose. I had a communication from a lady who was on a steamer in the Mediterranean, and she said that she had seen a very fine stream of meteors on Nov. 14 between Gibraltar and Malta.

Dr. Stoney. I have two interesting letters which have reached me recently, one being from Mr. Anderson at Madeira, who observed there on the 14th, and he gives a list of the number of meteors he saw, and a diagram of the frequencies, which shows a very marked maximum between 5 hours and 5.30. The other letter, from Mr. Willoughby Meares at Calcutta, states that he observed on the 14th, and therefore several hours before observations were possible here. He writes:—"Up till 14 hours, I was only keeping a casual look-out at my telescope; but until then there were very few meteors visible, and only one bright one from Leo. Between 14^h 30^m and 16^h I saw 12 meteors: many of the other meteors noticed came from a point in Leo, but they were not so bright and did not leave lasting streaks." The hours recorded are in Calcutta local time.

The President then called on *Mr. Stone* to give an account of the recent Eclipse expedition to Novaya Zemlya.

Mr. Stone. About the middle of June, 1896, Sir Geo. Baden-Powell, with great public spirit, intimated that if observers were prepared to accompany him, he would place them, by means of his yacht 'Otaria,' in Novaya Zemlya on the line of totality of the Solar Eclipse of Aug. 8. I had not intended taking any part in the observations, but the consideration that Novaya Zemlya was widely separated from the stations already selected by intending observers, and that a fair chance of success presented itself, influenced me in accepting Sir George's kind offer.

The very brief interval available did not permit of the preparation of any elaborate apparatus: but Mr. Hilger constructed for me a special spectroscope, by means of which direct eye-observations could be made and photographs taken simultaneously.

The telescope to which this spectroscope was attached was a small equatoreal belonging to the Radcliffe Observatory, with an exceptionally good object-glass of 3·3 inches, and with means of adjustment in latitude, but with no clock movement.

In addition to this I provided myself with a good chronometer, a Richard self-registering thermometer, dry- and wet-bulb thermometers, aneroid barometer, and some diagrams specially prepared for sketching in the extent of the outer corona.

The 'Otaria' sailed from Dundee on July 16, but adverse winds and an accident to the piston-rod of the engine caused some delay. Hammerfest was reached on July 29, where Mr. Shackleton, observer to the Solar Physics Committee, was taken on board. Sailing thence direct for Novaya Zemlya, the ship arrived in sight of the island on Monday morning, August 3.

A station was selected on an uninhabited island about 72° 22′ 40″ N., and $3^h\ 30^m\ 33^s$ E., and near to the Samoyede station of Kamakula. It was found that a Russian party of astronomers and physicists were already stationed at Kamakula, and Dr. Backlund, of Poulkova, Prince Galitzin, and the other members of the staff warmly welcomed the British party.

The landing of the instruments and stores and the construction of the tents and hut were proceeded with as rapidly as possible; but the preparations were only concluded late on the Saturday night, Aug. 8.

On Sunday morning, at 2 o'clock, the Sun was shining, and final arrangements were made for the Eclipse. Just before totality the passing of a dense cloud prevented a photograph being taken of the low reversing layer with the slit spectroscope; but when the cloud had passed over the spectroscope was turned on a large prominence. The two images formed by red hydrogen and yellow helium were distinctly seen, and less perfectly the image in the blue hydrogen and another in the red beyond the hydrogen line C. A photograph was secured of this prominence. The spectrum of the inner corona was examined and notes made that three red lines were distinctly seen, one of which was at the extreme red end of the field of view of the spectroscope. This line is found, by examination of the spectrum of helium with the same spectroscope, to be helium.

The telescope was then turned on the outer corona, and moved away from the Sun until nothing was visible in the spectroscope, but a continuous spectrum and the line 1474, which appeared much fainter than at the Eclipse in 1874. A photograph was then taken. During the 30 secs. exposure, I examined the eclipse phenomena, and remarked the general likeness of the outer corona to that seen by me in 1874, but that its colour was steel-blue instead of a faint green. Venus, Jupiter, and Mercury were at this time distinctly visible. After totality had passed, two plates were successively exposed for 30 secs. each to that portion of the sky where the outer corona was known to be.

Sketches of the extension of the corona were made by several observers, and photographs were taken by Sir Geo. Baden-Powell and Mr. Shackleton. The approximate times of totality were noted as: beginning $7^h\ 34^m\ 54^s$, and ending $7^h\ 36^m\ 44^s$; duration $1^m\ 50^s$. This is a general account of the work done during the eclipse.

Meteorological observations were taken on days before and after, also during the eclipse. At the time of the eclipse the humidity was about 77, and this was about the driest of any day that the party was at Novaya Zemlya; the other days vary from 80 to saturation. A great fall of temperature was felt during the time of eclipse, and after the eclipse about 10°, from the formation of cloud.

The photographs obtained have since been carefully measured. Besides the two pictures of the prominence which were seen in the rays C and D_3, there are seven images which admit of measurement, and one visible, but not accurately measurable, in the photograph of the prominence.

A scale was prepared for the spectroscope from photographs of hydrogen and helium in vacuum-tubes, and by measurements of solar spectra taken before and after the eclipse expedition. The scale thus formed allowed of a clear identification of the prominence spectra; and a curve was then formed with wave-lengths as ordinates and the measured distances in millimetres as abscissæ: it was found that a continuous curve could be swept through all the measured points.

It appears that the prominence which is just visible on the negative near the red end is helium D_3, $\lambda=5876$, which was seen in the eye spectroscope.

The combined observations of the eye spectroscope and spectrographic plate give, therefore, prominences for all the bright hydrogen lines α, β, γ, δ, ϵ and α', and the helium lines 5876 and 4472. There is also a well-marked prominence formed by light of about the wave-length 3934, which is that of the well-known K line usually attributed to calcium; but if calcium was present in the prominence there is no reason for attributing the prominence of wave-length 3970 (ϵ) solely to hydrogen, and it must be regarded as the combined effect of calcium and hydrogen, and I have been much surprised not to find an image of the prominence corresponding to the wave-lengths of any of the other bright lines in the spectra of calcium; and, without attempting to express at present any definite opinion on the point, it appears to me possible that the wave-length 3934 may be sensibly that of some hitherto unrecognized gaseous line. There is one point in connection with these observations of spectra of the prominence which struck me at the time and has been strengthened by an examination of the photograph; but without attempting to assert that the images formed by helium D_3 and hydrogen C were identical in form and extent, their similarity was certainly very striking, and it led me at the time to infer that this identity of form must arise from a sensibly uniform distribution of hydrogen and helium throughout the upper portion at least of the prominence.

The photograph of the outer corona has not yet been fully discussed, but it shows faint traces of three absorption lines about $\lambda = 3799$, 3934, 3970, a powerful general absorption or absence of continuous spectrum for some distance beyond H, then a continuous spectrum nearly to F; a deep band from about λ 5050 to 5889; then a bright continuous spectrum. Probably some of the results are influenced by the mode of preparing the isochromatic plates and the wave-lengths to which the plates are sensitive.

The two photographs taken after the end of totality, show but slight differences from that taken during totality, except that the Fraunhofer lines K, H are more intense, and some fainter lines are just visible in other parts of the spectrum.

Mr. Shackleton then exhibited on the screen photographs taken by himself during the eclipse. The most important photograph shown was one of the "flash," taken at the instant of totality, and showing a vast number of bright lines in the form of arcs of different lengths. A portion of this photograph was also shown enlarged for comparison with the ordinary Fraunhofer spectrum. A photograph of the 1474 K ring, taken also with the prismatic camera, was shown and compared with a photograph of the corona itself.

Sir George Baden-Powell. I really feel that after the excellent description of the scientific results obtained by the expedition given by my friends Mr. Stone and Mr. Shackleton I should be intruding. [Cries of "No."] It is always necessary, even in the most scientific matters, to understand the conditions under which the observations were taken. In the first place, it was necessary, in my opinion, to get some qualified astronomers with their instruments to Novaya Zemlya. I happened myself to wish to go there for other purposes connected with meeting my old friend Dr. Nansen. Hearing that I could be of some service to science

I was glad to devote all the abilities I possessed, and my ship, to taking these astronomers there. We had some difficulty in getting there in time, but, as has happened before in certain things we have undertaken in that good ship, we got there well in time. I did think before I left that there was something in the argument put forward that going with these large instruments to so high a latitude we should possibly obtain results which would be valuable, even supposing other stations obtained clear skies. There is something which we have not yet investigated thoroughly in the atmosphere of the high latitudes of the Arctic regions, and I was carried back to results obtained in astronomy by Prof. Piazzi Smyth in the higher atmosphere at Teneriffe, and I could not but feel a satisfaction that we should attempt observations in an untried part of our planet. When we got to our station, we found there were certain climatic difficulties to be faced, and mastered if possible, before we got to actual observing; and I assure my hearers that if they had been there they would agree with me and my wife who was present throughout that no persons were ever so heroically devoted to their duty as Mr. Stone, Mr. Shackleton, and Mr. Webb. They set up instruments of a most delicate character in a state of atmosphere which I can only describe as continuous hail, rain, fog, and wind. The temperature seldom rose above 40°, and every credit and praise is due to those astronomers for what they did in getting those instruments into position and adjusted in spite of these difficulties.

Now we come to the day itself. Mr. Stone has mentioned the unpleasant proximity of wild dogs, but he has not told you of the very severe storms and gusts of wind that blew, nor of the cold weather; but in spite of all this Mr. Stone and Mr. Shackleton slept ashore in a very rude shanty, which we made as comfortable as possible the night before the eclipse, so as to ensure the safety of the instruments. We were a party fourteen strong, and at 5 o'clock in the morning we were all there in time. Mr. Stone and Mr. Shackleton had been up pretty nearly all night, making various adjusments of the instruments and otherwise working to secure success. They entrusted to my charge the coronagraph, and all the members of my crew not only took their part with extraordinary willingness, but also showed great delight in what they considered an interesting observation, considering they had their reward in being allotted positions around the instruments under the command of these distinguished astronomers. Right at the last moment, however, the siderostat would not work. It had been set up and thoroughly adjusted, but at the last moment, when the Moon had got a long way over the Sun, it would not work; and looking through the coronagraph telescope I saw the image pass steadily but surely across that bit of ground-glass which we all watched so anxiously, and nothing that the devoted energy of Mr. Shackleton could do would make it work properly. This happened almost up to the point of totality, but, with the dash of genius, Mr. Shackleton certainly went for that siderostat with what we uninstructed persons would call rashness, and dashed back into the hut saying "that will do," and it did do. I saw the image in the centre of my ground-glass, which I had then to take out and begin the series of photographs I had been instructed to take at various intervals during totality; and I think one of the happiest moments in my life was when somewhat dazed, because immediately after totality the burst of sunlight was so great, I had sufficient senses left to slip in the ground-glass to see where the Sun had got to, and saw our old friend right in the centre of the field. I assure you the conditions under which these observations were made were very trying ones. We had our work cut out for us, to get the instruments and the astronomers there, but we did it, and in plenty of time. What happened afterwards was due to the energy and skill of the astronomers. They kindly instructed the members of the crew—myself amongst the others—what we were to do to help, and we did our best. I think Mr. Stone showed excellent judgment in not telling the crew where to expect to see stars. They were told to look for stars and report them afterwards. These seamen are trained in look-out duties, but they are not trained sufficiently to know even the difference between a planet and a star, and it is most remarkable that every member of the crew coincided exactly in the observations he had taken of the stars. Now a word as to the drawings. We had a hut a mile and a half from the station and the men were especially instructed to draw what they saw, and pledge themselves not to touch up their drawings afterwards by comparison with what others had drawn. So that the samples you have seen are true samples of what these men actually saw, and I think in that sense they are valuable. The conditions under which the observations were taken were, I admit, very favourable. I may be permitted to add that in my opinion—although it is not usual to introduce politics here, but I happen to be a member of Parliament—I do think that for a nation of taxpayers, who pay something like one hundred millions of taxation every year, that the paltry sum, which I believe was all that was granted, of £1,200 to view so important a scientific event as the total eclipse of the Sun was strangely inadequate. I am not going to say whether that sum would have sufficed to take an expedition to Novaya Zemlya — I know what those expeditions cost; but I do say for myself—and I have a special command to do so for my wife—that if ever a yacht is wanted to take willing people to see another eclipse they will know where to find it if we are free then. It may perhaps be a little vanity on my part, but I think that our expedition to Novaya Zemlya has in two points done good work. For the one point we deserve not credit but blame, and that is for our somewhat unchristian joy at the fact that although we saw the eclipse most beautifully no one else saw it. The other point I would mention is that I am very happy to have been a humble agent in securing these admirable photographic results, because I understand that photography, as applied to total eclipses, was not really very successful until 1893, and that in the coming eclipse of 1898 the results to be obtained then will be all the better for the fact that we have placed, as it were, a stepping stone between the eclipses of 1893 and 1898 by which learned astronomers and scientific photographers will be greatly assisted when the eclipse of 1898 comes. [Sir George Baden-Powell then showed, on the screen, photographs of the corona of the Eclipse of August 1896, and also that of the Eclipse of 1886 for comparison, and some pictures relative to the expedition.]

The President. This does not seem to be a paper that calls for any controversial remarks, and I am sure you will all join with me in returning your very best thanks to Mr. Stone, to Mr. Shackleton, and particularly to Sir George Baden-Powell. [Applause.]

Admiral Sir Erasmus Ommanney As a veteran Arctic voyager, I cannot help rising to express my admiration for the noble conduct of Sir George Baden-Powell in venturing in the vessel he did to Novaya Zemlya. I can understand exploring in the Arctic regions in a vessel adapted for the navigation of those seas; but I think it a most gallant and unprecedented thing for Sir George Baden-Powell, in a vessel not adapted for the work, to have gone on this expedition, and have conveyed our astronomers to Novaya Zemlya. Without Sir George Baden-Powell the whole world would have been without these very skilful and scientific observations. I am sure that the whole world of astronomers in general are deeply indebted to him for his gallant and disinterested conduct in undertaking this expedition; and I hope the Council will convey their appreciation of his services by some communication to him expressing the thanks of the Society.

The President. I am very glad of the assistance that Admiral Ommanney has given me in making the vote of thanks of this meeting a little more pronounced than I could do myself. It was not a point which struck me that the good ship 'Otaria' was unfitted for Arctic work. That only increases the obligation that we are under to Sir G. Baden-Powell. There is not the slightest doubt but that he has saved the situation for British astronomy —not but what we think that science is altogether cosmopolitan, but in our hearts we are more than satisfied that England has done something. I have heard that an observer, not an Englishman, who had been at a station where observations were made, on coming to a port heard the news of what the other astronomers had done, and was seen leaping wildy and gesticulating. When he was asked the cause of this hilarity, he said: "Vadsö notings, Japan notings." This feeling is very natural, but I am sure none of us like to see it carried to extremes. However, I join in Sir Baden-Powell's remark when he says that he is proud that he has done something for English astronomy, and I am sure you will all join in thanking him, and Mr. Stone and Mr. Shackleton, for the very charming way in which they have given us an account of their expedition. I can almost feel myself going with them—even to the stoppage of the clock of the siderostat. I will ask you to return your most hearty thanks to Sir George and Lady Baden-Powell for the excellent service they have rendered to astronomy.

The President. While we are on the subject of eclipses, perhaps you will allow Mr. Wesley to make a few remarks on the shape of the corona.

Mr. Wesley. Sir G. Baden-Powell showed a copy of the 1896 corona taken at Novaya Zemlya, and by the side of it a copy of the drawing of that of 1886, which showed, what we have all noticed, the extraordinary symmetry and extraordinary resemblance between two eclipses taken ten years apart. I thought it would be interesting under these circumstances to call attention to an eclipse which happened 11 years ago, and therefore would seem more properly to compare with this, but of which very little is known. Here is a rough tracing from a sketch, I made some years ago from a photograph, of the outline of the corona as seen during the eclipse of 1885. This remarkable extension with double curvature seems at first sight to correspond to a similar streamer of the corona seen at the recent eclipse; but if the orientation of the sketch is correct, its position as measured from the solar pole is quite different, so that the corona of 1896 is decidedly less like that of 1885 than it is like the corona of 1886. I wish also to call attention to the peculiar detail in the south-east quadrant, which is decidedly unusual. There are also details in the coronal structure which seem to point to a connection between the corona and the prominences.

The following papers were announced and partly read:—

W. H. M. Christie, E. H. Hills, and H. H. Turner. "Report on the Expedition to Japan to observe the Total Solar Eclipse of 1896, Aug. 9."

A. A. Common. "Report on the Expedition to Norway to observe the Total Solar Eclipse of 1896, Aug. 9."

W. F. Denning. Real Paths of 101 Meteors observed during the ten Years ending November 1896."

G. von Niessl. "Catalogue of real Paths of large Meteors."

A. Marth. "Ephemeris for Physical Observations of the Moon, 1897."

John Tebbutt. "Observations of Minor Planets at Windsor, New South Wales."

A. M. W. Downing. "Comparison of the Sun's Longitudes for 1901, computed from Newcomb's Tables of the Sun, with those computed from Le Verrier's Tables."

G. Johnstone Stoney. "Ephemeris of the Leonids."

A. A. Rambaut. "On the Inequality due to refraction in the Apparent Diurnal Motion of Stars; and a Method of allowing for it in Astronomical Photography."

A. A. Rambaut. "Observations of the Leonids on 1896, Nov. 13 and 14."

T. H. Safford. "On the various forms of Personal Equation."

Royal Observatory, Greenwich. "Observations of Comet *f* 1896 (Perrine)."

J. L. E. Dreyer. "On Systematic Errors in observing Right Ascensions of Nebulæ."

T. W. Backhouse. "The Aurora of 1896, March 4."

Radcliffe Observatory, Oxford. "Observation of Brooks' Periodic Comet, *c* 1896 (1889 V.)."

E. J. Stone. "Observations of the Solar Eclipse of 1896, Aug. 8–9, at Novaya Zemlya." Communicated by the Joint Permanent Eclipse Committee.

The following gentlemen were elected Fellows of the Society:—

G. A. S. Atkinson, 238 Newport Road, Cardiff; *J. B. Trivett,* Trigonometrical Survey of N. S. Wales, Sydney.

The following Candidates were proposed for election as Fellows of the Society:—

Geo. Banks, 4 John Street, Bedford Row, W.C. (proposed by C. A. Taylor).

J. C. Clancey, F.R.G.S., F.S.I., Assistant Director of Land Records and Agriculture, Burma (proposed by John Coles).

W. F. Doak, M.A., Assistant, Nautical Almanac Office, 3 Verulam Buildings, Gray's Inn, W.C. (proposed by A. M. W. Downing).

Rev. E. S. Leverton, M.A., School House, Kirkham, Lancashire (proposed by E. Dunkin).

E. F. J. Love, M.A., Lecturer in Physics in the University of Melbourne (proposed by Sir R. S. Ball).

W. A. Smith, Merchant, 74 Hagley Road, Edgbaston, and 94 Charlotte Street, Birmingham (proposed by W. J. Lancaster).

Edward Weldon, Merchant, 10 Morden Road, Blackheath, S.E. (proposed by H. P. Hollis).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The first ordinary Meeting of the current Session of the British Astronomical Association was held on Wednesday, November 25, *Mr. N. E. Green, F.R.A.S.*, the newly-elected President, in the Chair. Sixty-six new Members were elected, and the names of a number of Candidates were read and suspended.

Two papers on the remarkable train of Sun-spots visible from 1896, September 9 to 23, one by *Miss Brown*, the Director of the Solar Section, and the other by *Mr. E. W. Maunder*, were read. Miss Brown pointed out that this remarkable outburst came at a time when a steady decrease in solar activity had set in, and more than three years after the date of maximum. The train was remarkable for its immense longitudinal extension rather than for the size of its constituent parts, which probably accounted for its rapid extinction and total disappearance. Mr. Maunder also called attention to the fact that this remarkable disturbance occurred at a time when the Sun's activity was fast declining, and added that it was the longest connected group of sun-spots within his own experience of solar study, namely from 1874 to the present time. By the permission of the Astronomer Royal copies of some of the photographs of this group taken at the Royal Observatory, Greenwich, were thrown on the screen. Mr. Maunder added that no unusual magnetic disturbances coincided with the appearance of this group.

The President said this train of spots had a special interest to him, as they afforded a singularly definite appearance of hollow and projection. It was difficult to say whether these appearances were illusions or not, but certainly the particular spot pointed out by Mr. Maunder seemed, when on the limb of the Sun, to have a distinct notch in connection with it, and on the other hand the faculæ close by had very distinctly the appearance of projection.

Mr. Maunder and *Mr. Chambers* each expressed his opinion that the inspection of Sun-spots certainly left a strong impression on the mind that they are hollows or depressions.

Capt. Noble, while not denying this, thought that the appearance of a spot as a notch in the limb did not prove this hollowness.

Mr. Schooling read a paper on "Aerial Spectres," by *M. Antoniadi*, of Juvisy. The writer had often tried to reproduce artificially the spectre of the Brocken, but without success. On October 30 last, however, during a dense fog, he was surprised, on stepping out on to the terrace of the observatory, to see his shadow some 200 ft. high thrown on the mist by a strong lamp. M. Flammarion and l'Abbé Moreux, who were present, both saw exactly the same appearance. A curious feature of the phenomenon was that each one saw his own spectre only.

Mr. Schooling also read a short paper by *Mr. A. Watson*, of West Malvern, describing his observation on Nov. 5, at about $4^h\ 15^m$, of a mock Sun. It appeared about four diameters N. by E. from the Sun, and was of the colour of the Moon as seen by daylight. This unusual phenomenon was seen by several other observers besides himself.

Mr. Crommelin gave a summary of two papers he had written. In the first, on "The Conjunction of Jupiter and Saturn in November 1901," he pointed out that it would be the closest conjunction of these two planets, with one exception, since the invention of the telescope. The only closer one took place as long ago as 1683. On that occasion the two planets approached one another within a distance of 11′, and there would not be another approach so close until 2020. The conjunction would take place on November 28, and the two planets would be then a little over

26′ of arc separate from one another, and they would remain within a distance of 32′ for a period of six days. They would be evening stars at the time, 38° from the Sun. The second paper was on "The Occultation of Mercury by Venus on May 17, 1737." This he believed was the only observed occultation of one planet by another since the invention of the telescope, and it was impossible to be sure in the cases recorded before the invention of the telescope that occultation actually took place, since the two bodies would appear as one when they were within a distance of 30 or 40 seconds of arc, or possibly more. This occultation was observed at Greenwich.

Mr. John Tebbutt contributed a paper recording observations of the planet Jupiter, and *Mr. C. Roberts* also sent a paper on the same subject.

The preliminary report of the Mars Section was also read.

In a paper on "The Canals of Mars observed at the Manora Observatory," *Herr Leo Brenner* stated that he had seen 101 canals up to date, among them 70 of Schiaparelli, 12 of Lowell, and 10 new ones,

An album of Jupiter drawings brought by *Rev. W. R. Waugh* was on view.

The Meeting adjourned at 7 o'clock.

ROYAL METEOROLOGICAL SOCIETY.

The monthly Meeting of this Society was held on Wednesday, Dec. 16th, at the Institution of Civil Engineers, *Mr. E. Mawley, F.R.H.S.*, President, in the Chair.

An interesting paper by *Dr. Leigh Canney* on the "Winter Climate of Egypt" was read by the Secretary. The climate of Egypt during the winter is influenced by the Libyan desert, by the Mediterranean Sea, and by the extent of cultivated land. The author gave the results of a series of observations which he had carried on during the past three winters. The observations were started with the object of arriving at a comparative knowledge respecting the climates of the various stations now considered as health-resorts in Egypt, and by a strictly comparable method to arrive at the precise differences between the climates of Upper and Lower Egypt, all previous observations having failed in this respect. The stations at which observations were made were Cairo, Helouan, Mena House Hotel, Luxor, Assouan, Valley of the Tombs of the Kings, and the crest of the Libyan Hills. As self-recording thermometers and hair hygrometers were used at each station, valuable data have been obtained on the diurnal variation of temperature and humidity.

Mr. R. H. Curtis also read a paper on an "Attempt to determine the Velocity Equivalents of Wind Forces estimated by Beaufort's Scale." The author has compared the anemometric records at

Scilly, Fleetwood, Yarmouth, and Holyhead with the wind forces as estimated by the observers at the same or adjoining stations, and has by this method obtained a satisfactory table of velocity-equivalents in miles per hour for the estimated forces by Beaufort's scale.

A new Class of Variable Stars.

Among the curiosities of the heavens few are more worthy of attention than a small star in the constellation Pegasus, the peculiarities of which were made known just a year ago. U Pegasi [*], discovered as a variable by Dr. Chandler in 1894, was at first taken to be of the Algol type; but its phases, on fuller enquiry, proved to be continuous. The star's brightness is always on the move; it has no halting-places: maxima and minima are equally sharp and short; and they alternate with astonishing rapidity. A complete oscillation is accomplished in 5½ hours, its amplitude being seven-tenths of a magnitude—from $9^{m}\cdot 0$ to $9^{m}\cdot 7$. In the course of two hours and 28 minutes this singular object loses almost half its light, which it regains in three hours and four minutes. Thus, contrary to the rule with short-period variables, the rate of increase is slower than the rate of decrease. The distinction is so marked as to imply an essential difference in constitution or mode of light-change. Stars that are periodical in a few days are all but unanimous in ascending to maximum at least twice as quickly as they descend from it. This manner of procedure is well illustrated in the following little Table drawn up by Dr. Chandler [†]:—

Name of Star.	Period in days.	Ratio of Time of Increase to entire Period.
R Muscæ	0·88	0·334
U Trianguli Australis	1·55	0·298
R Trianguli Australis	3·39	0·245
Y Carinæ	3·64	0·253
W Carinæ	4·38	0·205
T Vulpeculæ	4·44	0·293
T Velorum	4·60	0·391
S Crucis	4·69	0·320
δ Cephei	5·37	0·300
Y Sagittarii	5·77	0·312
R Crucis	5·83	0·240

On an average of these stars, the intervals from minimum to maximum, and from maximum to minimum, are in the proportion of

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[*] Numbered 8598 in Chandler's "Third Catalogue of Variable Stars," Astronomical Journal, No. 379.

[†] Astr. Journ. No. 374.

0·29 to 0·71, and Professor Pickering mentions that, in No. 18 of the long list of variables detected by Mr. Bailey in the globular cluster Messier 5, the same ratio is 0·17 to 0·83. In U Pegasi it is 0·55 to 0·45; the balance, if not equipoised, inclines the other way.

On this ground alone, Dr. Chandler set it apart from ordinary variables such as R Muscæ, the southern revolving light, which swings to and fro across the limit of naked-eye visibility once in twenty-one hours. Two other stars promptly ranged themselves beside U Pegasi. Professor Paul's remarkable variable, S Antliæ, was assigned a place among the members of the Algol family in 1888, and kept it until 1896. It changes from 6·7 to 7·3 magnitude in seven hours forty-seven minutes, during less than one-half of which brief term it was supposed to remain uneclipsed. But a recent series of photometric measures at Harvard College have proved that it fluctuates without interruption. "From this it appears," Professor Pickering writes, "that S Antliæ is not a star of the Algol type, and that it belongs to the class of variables of short period like δ Cephei and η Aquilæ. An interesting feature of its curve," he adds, "is that the time of increase occupies 0·62 of the entire time of variation." The object, then, is strictly analogous to U Pegasi, while between eight and nine times as bright.

A third star has been added to the new class by Mr. A. W. Roberts, of Lovedale, South Africa*. This is Lacaille 5861, found by him to vary slightly, but surely, in seven hours and sixteen minutes. It ranges, in fact, only from 7·47 to 7·81 magnitude. The representative curve appears to be perfectly symmetrical, and is traversed with unfailing regularity. The star is situated in Centaur. Mr. Roberts regards the species to which it belongs as essentially characterized by a narrow scope of rapid change, and remarks that their standing is intermediate between variables of the δ Cephei and of the Algol kinds.

Resuming the particulars so far learned about the trio, we have S Antliæ varying to the extent of $0^{m}{\cdot}60$ in $7^{h}\ 47^{m}$, 0·62 of which time is spent in the rise from minimum to maximum; Lacaille 5861, with a range of only $0^{m}{\cdot}34$, in a period of $7^{h}\ 16^{m}$, equally divided between increase and decrease; and U Pegasi, oscillating by $0^{m}{\cdot}70$ in $5^{h}\ 32^{m}$, and showing a very slight retardation on the ascending side. Thus in one of the three stars the balance of time is held even between loss of light and recovery; in the others there is evidence (which may or may not be substantiated) of a somewhat tardy return to full lustre. In several of the Algol stars, however, notably in S Cancri, U Coronæ, and δ Libræ, such a delay is quite conspicuous; and it cannot be attributed to the eccentricity of their orbits, since no instance of accelerated recovery is known, and the cause in question should work indiscriminately either way. The light-curves of U Pegasi, S Antliæ, and Lacaille 5861 accordingly represent perfectly the *minimum phases* of Algol stars. The fact is also significant that none of them decline to the full extent of halving their brightness—that is, change by as much as 0·75 magnitude—although U Pegasi grazes that limit. This strongly suggests mutual eclipses, total or partial, by pairs of equal stars, revolving in a period double that of their light-change. The eclipses, it is true, should be perpetually in progress. Except at their actual elongations, corresponding to maxima of lustre, the disc of one of the pair—as viewed from the Earth—should always to some extent encroach upon the disc of its companion. In other words, they should revolve almost in contact.

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* Astr. Journ. Nos. 378, 384.

There is every reason to believe that such systems exist. If Dr. See's theory as to the origin of binary stars be sound—and no flaw has yet been found in it—they must have a settled place in the scheme of things. They illustrate the initial stage of approved Algol variables, composed, presumably, of two equal luminaries, such as U Ophiuchi and Y Cygni. Nay, Mr. Roberts has discovered, in X Carinæ, a star ranked by him as of the Algol type, but probably of an intermediate character. Its minima recur at intervals of thirteen hours; and they are impossibly long, except on the supposition made by him that the orbital period includes two light-periods. The minimum is just one-half the maximum brightness, so that the eclipsing stars are sensibly equal, and revolve in a plane that may be called coincident with the line of sight.

It is difficult to resist the persuasion that X Carinæ has a less mature analogue in U Pegasi. That it is a binary star will scarcely be denied; the only question that can be raised is whether its variability is due to eclipses or to other, as yet inscrutable, causes, such as operate in δ Cephei and its congeners. But, as we have seen, the fluctuations of U Pegasi are of a different type from those of δ Cephei. On the supposition that it is really an Algol star with the stationary maximum abolished by the proximity of its components, let us see what kind of system it constitutes.

In the first place, since the minima are of equal duration and equal intensity, the eclipsing bodies are of equal size and lustre. Their eclipses are virtually total, the loss of light falling very little short of one-half; and their necessarily circular orbit, accordingly, is cut across by the visual ray.

It is traversed in $11^h\ 4^m\ 30^s$. Assuming that the joint mass of the stars is equal to that of the Sun, the radius of their orbit is 1,086,500 miles; and the gap dividing them being, by hypothesis, relatively small, a diameter of one million miles may be assigned to each of the shining globes. Their density will then be less than one-fifth the solar mean density, or about a quarter that of water. This does not seem to be an improbable degree of rarity. And it continues the same on any assumption whatever as regards their mass. For, since the linear dimensions of an orbit vary, the period remaining the same, as the cube-root of the mass of the system, the diameters of the spheres composing it, and revolving approximately in contact, vary in the same proportion. That is to say, their bulk retains a constant proportion to their mass, and their density consequently remains unaltered. This is a curious result of the arrangement just described, and may serve to test its possibility. Still more important is the circumstance that it involves very swift orbital movement, which, in totally eclipsing stars, such as we suppose U Pegasi to be, would, twice in each period, at the brief maxima of brightness, be directed wholly in the line of sight. The velocity of U Pegasi, admitting each component to be half as massive as the Sun, would amount to 170 miles a second; and the spectroscopic effect would be doubled by opposite line-displacements. The spectral rays would then separate, at maxima, to an extent corresponding to a velocity of 340 miles a second. The phenomenon, even in a ninth magnitude star, ought to be conspicuous on a photographic plate exposed with a large objective-prism. It is, indeed, likely to be present, in a measurable degree, whatever be the nature of the star; yet its punctual reproduction at maxima would attest, with virtual certainty, the occurrence of eclipses at minima.

The orbital velocity of S Antliæ comes out 153 miles a second, and the plane of movement can be inclined to the visual ray only by a few degrees, the eclipses—if eclipses truly take place—being nearly total. The relative line-displacements would hence correspond to a speed of about 300 miles a second. They ought to be easily observed, since the star rises above the seventh magnitude once in less than eight hours. Its 28° of southern declination renders it, however, an unsatisfactory object in these latitudes. Its mean density would, on the present hypothesis, be exceedingly small—no more than one-seventh that of the Sun. The circumstances of Mr. Roberts's star in Centaur are such that it might almost be designated a twin system of S Antliæ; only the plane of its revolution lies farther from coincidence with the line of sight. The evidence of the spectroscope as to the construction of these remarkable systems will surely not long be wanting. Whether positive or negative, it will be of the utmost value.

AGNES M. CLERKE.

Duration and Character of the Leonid Radiant.

ONE interesting feature of the Leonids is the long duration of the shower. Meteors continue to fall from the well-known radiant in the sickle from November 10 to 20, and probably a close watch on the morning hours of November 8 and 9 would reveal distinct traces of the display; but I have never thoroughly investigated this shower as regards its duration and the position of its radiant from night to night.

In the next few years great attention will be given to the

Leonid meteor-stream by the general public as well as by astronomers, and it seems desirable to point out to the latter that useful observations may be secured on the nights immediately preceding and following the date of maximum abundance. It will be important to learn the visible duration of the shower, and whether the radiant retains a fixed position amongst the stars or moves eastwards 1° per day like the Perseids. My Leonid radiants do not prove much in reference to the character of the radiation, but they indicate that the activity of the shower is extended over 11 nights :—

			α.	δ.
			°	°
1877	Nov.	10–13	148	+24
1879		13	148	+23
1888		13	149	+22
1885		14	149	+21
1887		14	150	+22
1896		14	150	+22½
1885		16	150	+22
1876		19–20......	149	+22

The last of these was not sharply defined and cannot be regarded as accurate. The positions show a slight increase in R.A., but it is quite possible the differences are due to errors of observation. In 1877, November 10, I suspected the place of the radiant to be at 149° +24°; if any weight can be attached to this and to the position determined on Nov. 19–20, it would appear that no displacement whatever occurs.

I have registered the following Leonids during the later stages of the shower's activity :—

No.	Date.		G.M.T.	Mag.	Path				Length.	Appearance.
					From		To			
			h m		°	°	°	°		
1...	1885 Nov.	16	13 59	4	146	+17	144	+14	3½	Sl., streak.
2...		16	14 37	3	155	+12	157	+ 8	4½	V. b. streak.
3...		16	15 32	4	168½	+50	179	+58½	11	Swift.
4...		16	16 22	4	161	+27	166	+28½	5	Sw., streak.
5...		16	16 29	2	173	+19	180	+18½	7	Swift.
6...		16	......	...	170	+33	177½	+36	6½	Sl., streak.
7...		17	15 24	3	181½	+48½	199	+52½	11	Sw., streak.
8...		17	16 9	2	183½	+33	194	+34	8½	Sw., streak.
9...	1876 Nov.	19	15 39	3	142	+25	136	+26	6	Sl., streak.
10...		20	15 52	3	186	+29	198	+29	11	Sw., streak.
11...		20	16 25	4	158	+42	161	+53	11	Swift.
12...		20	16 25	3	166	+32	174	+37	8	Sw., streak.
13...		20	16 42	4	149	+13	150	+ 9	4	Sl., b. streak.
14...		25	15 26½	4	149	+10	147	+ 4	6	

There can be no doubt that the streak-leaving meteors observed in 1876, Nov. 19–20, were true Leonids, their paths being near the radiant. Nos. 9 and 13 were very close to it, and presented the appearance of being much foreshortened.

No one but those who have narrowly and repeatedly watched the region of Leo in the morning hours at the middle of November would form a just conception as to the large number of meteoric radiants which exist in this quarter of the sky and are contemporaneous with the Leonids. I give below a selection of the places of these minor showers arranged according to R.A., but there are many others which remain to be determined:—

Circum-Leonid Radiants, November 10–15.

°	°	°	°	°	°
120	+15	136	+ 8	157	+74
121	± 0	141	+27	166	+ 3
124	+55	143	+50	166	+31
125	+40	146	+ 8	170	+47
132	+21	148	−12	177	+10
133	+31	154	+41	184	+29
133	+48	155	+12	190	+21
133	+70	157	+49	191	+58

Nearly all of these yield swift meteors with streaks like the Leonids. The difficulty in attributing every meteor observed to its correct radiant can therefore be readily imagined, for a path when prolonged in the backward line of flight will generally cut through two or three radiants. It is this complication which shows the necessity for great exactness in recording the apparent directions of motion and features of appearance in all cases, and proves that a considerable amount of practice is required on the part of observers to ensure precision.

It is natural to find gentlemen who have never yet determined any meteoric radiants speaking of the very unsatisfactory method of fixing these positions, and suggesting the photographic method as the only one likely to yield very accurate results. A quarter of a century ago it was thought pretty good work to get a radiant within 8° or 10° of probable error; now, however, we have confidence in our positions to within about 1½°, and it is not likely that photography will carry us much nearer the goal of accuracy. A meteoric radiant is not a point in the sky which can be defined like the stellar nucleus of a comet; it is often an area covering several degrees, and the meteors near it, being often curved and "brushy" in appearance, fail to indicate the centre very satisfactorily. Attempts have been made in several years to photograph the Perseid radiant, but in vain, and it looks probable that this method of getting the paths will only thoroughly succeed during a great meteor storm such as the Andromedes may be expected to form in 1898 or the Leonids in 1899. At any rate, it would seem from previous experiments that the minor showers are beyond its operations, and that we shall have to rely for some years to come upon the ordinary eye-determinations. No one will welcome more enthusiastically than meteor observers the employment of means which will at once relieve them from their protracted and sometimes tedious vigils and ensure a degree of accuracy which they could never hope to attain; but it will be really a disappointment to them if they find, as they appear likely to do, that too much has been claimed for the photographic method.

Bristol, 1896, Dec. 6. W. F. DENNING.

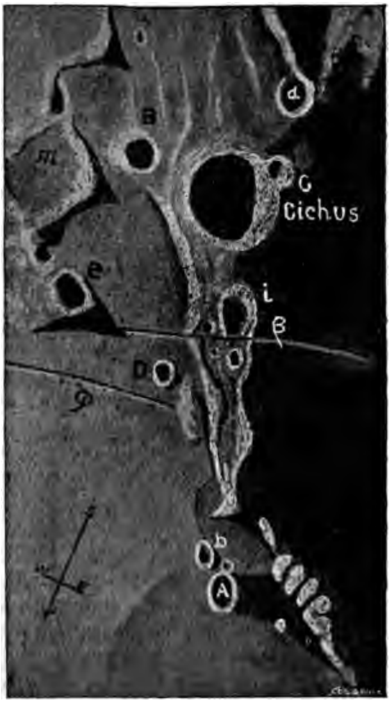
Selenographical Notes.

OBSERVATORY ATLAS OF THE MOON.—Through the courtesy of Prof. Holden I have received the first instalment of a work, which, when complete, will not only be a lasting memorial of one of the greatest photographic achievements of the great 36-in. refractor at Mt. Hamilton, but will certainly mark a new departure in the study of the physical features of the Moon, leading assuredly to a more accurate knowledge of its manifold details. There are few observers who are not more or less familiar with the admirable photographic reproductions, by Dr. Weinek and others, of selected portions of the lunar surface, and who have not often wished that work so excellent and valuable could be extended so as to include all the visible superficies. This desire is now in a fair way of being realized in a very perfect manner, so that before long we shall possess a picture of the face of our satellite more complete and true to nature than any representation we are likely to obtain of our own globe, until that problematical time arrives when some scientific Hans Pfaall shall succeed in photographing a cloudless Earth from the summit of a lofty peak on the rampart of Ptolemaus or other suitable standpoint on the Lunar World.

So common is the fact, that it seems trite to observe how peculiarly fortunate our Trans-Atlantic friends are in having in their midst so many public-spirited men and women, who, with a keen desire to extend the bounds of natural science, are ever ready to furnish the necessary funds for effecting this object, to Institutions or Corporations that have given proofs of their capacity to make good use of these advantages. A lengthy catalogue of these "endowments of research" might be made, were not the facts so widely known as to render it unnecessary. This Atlas of the Moon, the outcome of one of the most recent of these gifts, is being prepared and published at the cost of Mr. W. W. Law, of New York City. When complete, it will consist of at least sixty plates, with index-map, made from negatives taken by the 36-in. telescope. It is on the same scale as the well-known Lunar Chart of Beer and Mädler,—viz., 38·36 English inches to the Moon's diameter, or one-half the size of the more recent Chart of Schmidt. Plate I. shows a portion of the 3rd Quadrant extending from the S. pole to about S. lat. 30°. The formations are shown under evening illumination,—the west wall of Maginus being on the terminator.

Cichus.—This very remarkable circular ring-plain is familiar to the generality of observers, though probably there are many who have not noted some of the interesting characteristics of the mountain-arm on which it is placed, or of its immediate surroundings. As regards the formation itself, the most striking features are the extreme regularity of its shape, and the large bright crater, C, which stands on the summit-ridge of the south-eastern wall, an object which Webb, from an examination of drawings by Schröter made in 1791 and subsequently, suspected it had undergone a great change in size, as the Hanoverian astronomer represented it as a comparatively insignificant little object, whereas Mädler shows it much as it appears at present, viz., a most conspicuous appendage to the larger ring. As Schröter, however, though as a rule a careful draughtsman, to whom more credit is due than has been vouchsafed to him by his successors, doubtless frequently drew such features in a conventional way, merely indicating their presence by a small circular figure with a black dot in the centre, little reliance can be placed upon the discrepancy. Neison states that the floor of Cichus sinks more than 4000 feet below the surrounding surface; but those figures, if the plateau on which the formation stands is meant, must be reduced, as it is raised considerably above the level of the Mare Nubium. However this may be, the crater C is of great depth, as it remains filled with black shadow some time after the sunlight has reached the eastern quarter of Cichus itself. When the interior of the larger ring is about one-third illuminated, faint indications of terraces are apparent on the steep inner slope of the eastern rampart. These may be seen at the phase represented in the accompanying drawing, viz., when the formation is on the morning terminator.

The plateau, extending from the small ring-plain B, northwards towards Mercator, and terminating in a foot-shaped projection, is a very curious formation. From B, to a point about 20 miles north of Cichus, its western boundary consists of a somewhat steep and regular cliff rising abruptly from the Mare, and presenting a slightly concave inflection towards the west. At this point, however, it suddenly changes its direction, though maintaining its general character, and bends somewhat more towards the east, terminating in the foot-shaped projection just mentioned. There is a somewhat similar cliff on the eastern side, the two cliffs enclosing a space recalling the shape of the Italian peninsula. This enclosure abounds in detail, the most conspicuous being the large incomplete ring on the eastern rim of the plateau, a little north of Cichus, a second smaller depression still further towards the north, some other crater-like objects of a more minute type, a quasi cleft running from the southern side of the incomplete ring to the western edge of the plateau, and many little ridges and hills. But by far the most noteworthy feature in connection with this remarkable plateau is the coarse cleft which cuts across it from west to east. It can be traced from a point a little north of the crater marked *e* in the drawing, through the plateau, over the plain lying between it and Capuanus, to the western border of this formation. This rill was described by me in 'The Observatory,' vol. xvii. p. 207, though I only incidentally referred to the great crack or fissure it has made in the plateau. A glance at the drawing shows that it has broken the line both of the western and eastern boundaries of the boot-shaped plateau in a very curious way, and, not only this, has apparently cut through and destroyed the extreme southern wall of the large incomplete ring which borders it on the south; so that the plateau looks deformed, like a child's map-puzzle, with a section omitted. This chasm with its continuation (marked *β* in the drawing) would seem to be very clearly of subsequent date to the formation it traverses. When observing it at 8^h on October 14th, 1888, with powers 284 and 340 on my 8½-in. Calver, the east long. of the morning terminator



being about $25^{\circ}\cdot6$, I noted a prolongation of its direction towards the east, beyond the terminator, in the form of a faint narrow line of light, evidently a similar appearance to that recorded by Trouvelot in connection with the ring-plain Eudoxus on Feb. 20, 1877. The accompanying drawing, which was made between $7^{h}\ 30^{m}$ and $8^{h}\ 30^{m}$ on the first-mentioned date, shows this marking as then recorded with the two eyepieces used. It will be noted that the streak appears to bend slightly towards the north. I may add that the same phenomenon has been seen on two subsequent occasions when the conditions of illumination and libration were practically the same; hence it is likely enough that it may often be observed at this particular phase. As to its cause, may it not be due to some kind of exhalation, overhanging the cleft, illuminated by the rays of the rising Sun? In some cases, as, for instance, the canal-like rill connecting Cardanus and Kraft, the raised banks bordering it show out as bright lines in the blackness beyond the terminator, but neither in Trouvelot's '*mur enigmatique*' by Eudoxus nor in the case in point are there any signs whatever of walls bounding the cleft.

The western section of the great Hesiod rill, ϕ, is always an easy object at the phase represented. It appears to be interrupted by the large mound close under the western cliff of the plateau. On several occasions, however, notably on Nov. 13, 1888, when the east long. of the morning terminator was $31^{\circ}\cdot3$, I have almost certainly followed it over this obstruction up to a small crater (obscured by shadow in the drawing) which lies between the mound and the plateau. Its course east of this, previously described, to the region N. of Capuanus, is always far less easy to trace.

Beaumont House, Shakespeare Road,
Bedford, 1896, Dec. 19.

T. Gwyn Elger.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comets due in 1897.

Gentlemen,—

The comet (1890 VII.) which was discovered by Dr. Spitaler at Vienna on the morning of the 17th of November in that year was computed by him to be moving in an elliptic orbit with a period of about 6·4 years, and to have passed its perihelion on the 27th of October. A return to perihelion will be due on the 11th of March next, and Dr. Spitaler (who is now attached to the Imperial Observatory at Prague) has published an ephemeris in Ast. Nach. No. 3370, by which it appears that the comet was nearest the Earth towards the end of September, and that the conditions were the most favourable for its visibility in that month and in October. It was an excessively faint object in 1890, and we must conclude that there is little hope of its being seen at the present return.

The other periodical comets due to return in 1897 are those known as D'Arrest's and Swift's, both of which will be in perihelion in the spring. The former was discovered at Leipzig on the 27th of June, 1851. It was a very faint object, and was found to be moving in an elliptical orbit with a period of about 6½ years. Accordingly it was observed again in the winter of 1857, but only in the southern hemisphere, at the Cape of Good Hope. In 1864 it was not seen, being unfavourably placed, but it was observed at the returns of 1870 and 1877. In 1884 it again escaped observation (little hope was felt that this would be otherwise), but it was seen again in the autumn of 1890 after the perihelion passage, which occurred on the 16th of September.

Swift's comet was first discovered by Tempel on the 27th of November, 1869: but as its periodicity was not recognized until after its re-discovery by Prof. Swift at the Warner Observatory, Rochester, N.Y., in October 1880, it is usual to call it by his name. The period is only about 5½ years in duration, so that an unobserved return must have taken place in 1875. Being unfavourably placed also at the return in 1886, it again escaped observation; but it was well observed at the last appearance in 1891, when it passed its perihelion in the month of November, the next being due in the forthcoming April or May.

Yours faithfully,

Blackheath, 1896, Nov. 28. W. T. LYNN.

Discovery of Sperra's Comet.

GENTLEMEN,—

In the note on the discovery of Sperra's Comet on p. 404 of the 'Observatory' there are one or two slight errors which you will perhaps allow me sufficient space to correct.

The facts relating to this observatory's connection with the matter may be very simply stated. Mr. Sperra's announcement was received by mail on the afternoon of Sept. 1. At that time I was on the ocean. The letter was addressed to me, and not to the Allegheny Observatory, and hence it was sent to my residence, where it was opened later in the evening by my family, and returned to my Assistant with the request that it should be forwarded to Harvard College Observatory This was done by the next mail on Sept. 2. Thus, although a search was made for the comet on Sept. 1, the announcement of its discovery was not held back for the result of this. I am, however, not quite prepared to admit that such a course would necessarily have been an unwise one.

I take this opportunity to correct another error which, although it obviously arises from a mere slip, may possibly lead to some misunderstanding. At the January meeting of the Royal Astronomical Society, according to the report in the 'Observatory' (vol. xix. p. 72), a paper of mine was quoted as authority for the statement that the Lick telescope has a very large amount of spherical aberration. Of course chromatic aberration was meant, as my paper mentions nothing else. A difference of focus of some three or four inches for extreme rays may fairly be regarded as very great, although it is proportionally no greater than for small telescopes; on the other hand, the Lick telescope has sensibly no spherical aberration. Yours very truly,

Allegheny Observatory, JAMES E. KEELER.
Pennsylvania, 1896, Dec. 3.

PUBLICATIONS.

THE LIFE AND WORK OF CHARLES PRITCHARD [*].—A literary critic once observed that readers of books may be divided into two classes: those who read for profit and instruction, and those who read for rest and amusement. We suppose amongst the latter are those who enjoy biographies. The one before us is divided into three portions, which may be said to represent the daily life, the theological writings, and the astronomical work of Prof. Pritchard. The fault we usually notice in such works is that so much is said that might profitably be left out, or so much is left out that might have been advantageously brought forward. The first of these two characteristics is very evident in the dull and tedious opening chapters in the work before us, for they are painfully padded and needlessly expanded by the introduction of irrelevant stories and the relation of personal episodes concerning the lives of others beside that of the one under consideration. For instance, what interest can the ordinary reader of the life of Prof. Pritchard be expected to feel in knowing that the grandmother of the author of the first memoir was an actress of great ability? so much so that she made "a majestic curtesy to the Princess, which brought down the house in thunders of applause." Such superfluities are wearying and depressing, and only serve to prejudice the reader against the remainder of the book; and we regret that this escaped the attention of the compiler before publishing the work.

The reminiscences of school life by the late Professor himself, and his life at Freshwater, may be enjoyed by his intimate friends and late pupils; but we think the real interest of the work commences at the fifth chapter, which is penned by Miss Ada Pritchard. Her style is at once refreshing and attractive, and the open and liberal way in which she describes the Professor's sincere acknowledgment of indebtedness to his late Senior Assistant,

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[*] 'Charles Pritchard, D.D., F.R.S., F.R.A.S., F.R.G.S., Late Savilian Professor of Astronomy in the University of Oxford: Memoirs of his Life, compiled by his daughter, Ada Pritchard; with an Account of his Theological Work by the Lord Bishop of Worcester, and of his Astronomical Work by Prof. H. H. Turner.' London: Seeley & Co., 1896.

Mr. Plummer (now at Liverpool Observatory), and the language by which she records it, would strike even the most casual reader as at once dignified and appropriate, without being patronizing or effusive. So, too, with the article upon his last years at Oxford. Here the same author is clear and impressive, whilst towards the close of the article she becomes picturesque and even pathetic in her language.

With respect to the theological section, which has not so much to do with the usual subject-matter of this Journal, we cannot raise issue on any point. The reference made by the learned writer—the Lord Bishop of Worcester—to Professor Pritchard's letter to Pusey is exceedingly *à propos*, as affording what we may suppose to be an excellent example of the innermost thoughts of the man, and also serving to illustrate his undoubted power of expressing himself intelligibly. It has been said of Pritchard that "what he meant he could write, and what he wrote others understood as he meant"; a faculty not possessed by many men. The reference also to his celebrated sermon before the British Association is well worth reading.

It is, however, with much more interest that we read the article concerning "The Astronomical Work" of the late Professor, and we found it almost difficult to lay the book down whilst we made notes as our reading progressed. The attention is at once attracted by Professor Turner's preface to his article, but we fail, after careful reading, to find any justification for the apologies he has so strongly expressed concerning the imperfections which may be expected to follow. The account opens with a description of the early astronomical life at Clapham; then one or two remarks about the setting-up of the Professor's first telescope at the Grammar School, which, as the writer observes, are of some interest, "as showing the state of practical optics at that time." Pritchard's share in arranging to view the Eclipse of 1860 in Spain next follows, and a diagram of what he saw adds additional interest to the subject. We think it would spoil the enjoyment of astronomical readers if more was said upon this the most interesting portion of the biography before us; but we must be allowed to mention our appreciation of the short but neat and explicit accounts given of the various pieces of work initiated by the Professor. Especially do we call attention to that of the 'Uranometria Nova Oxoniensis.' The reference here made to the friendly but animated dispute which arose concerning the scale values of the wedges employed, and the method adopted to obtain them, between the Professor and another Fellow of the Royal Astronomical Society, is managed with much consideration; for, whilst relating most fairly the whole subject in a condensed form from beginning to end, Professor Turner has clearly set forth what we believe was the ambition of both controversialists from the first, that the question at issue should be handled by each advocate as much in a friendly spirit as in a strictly scientific one.

Here we must close our remarks, and we lay down the book with the conviction that, notwithstanding our strictures on the early chapters, we can honestly recommend it as a whole to readers, theological and astronomical, who care for biographies. The printing and general appearance of the book, including the photograph of the Professor, leave little to be desired, and we congratulate the several authors and the compiler on the completeness of the work. E. J. Spitta.

The Autobiography of Sir George Airy.—If it be held that the biography of a man should show his character, and should be a record of his actions with the attendant circumstances from which his thoughts and opinions might be deduced, in the book* now before us there is something wanting. This work of four hundred large octavo pages is a simple history of the events in the long life of a man who lived entirely for his work, and did a great deal more of it than most men. The history is typical in one sense of Airy, for it is orderly in the extreme. But a book written in this orderly way is hard reading. After a short personal sketch of the subject of the volume written by the Editor, his son Wilfrid Airy, the book goes on with a kind of diary written by Sir George, divided after the year 1828 into sections, each giving a *précis* of his occupation during a year, which, for the years after he came to Greenwich, are practically reprints of parts of the Annual Report of the Astronomer Royal to the Board of Visitors, with additions relating to more private matters; and these sections are interspersed with letters, some on scientific subjects, others purely personal written to members of his family.

As a bare statement of facts this book is valuable; the parts referring to his life and connection with Cambridge University, and his letters on the subject of the Cambridge examinations, will interest those who know the University. It will be valuable also to astronomers and especially to those who are acquainted with the inner working of Greenwich Observatory. But we doubt whether it will be found interesting as a book for general reading. The casual reader will scarcely care to know that "we stopped at York; went to the Tavern Hotel. In the morning (Friday) went into the Cathedral, &c.,"—for these things might have happened to an ordinary person, and in the autobiography of an extraordinary man one expects extraordinary events; and there are many letters in the book, the tone of which may be judged from the above extract. Airy lived at a time when science was largely developing and was beginning to be applied to the facts of everyday life, and he took his share in this development. He was one of the few men to whom the Government could turn for an authoritative and unbiassed opinion on subjects which may be called scientific. He was called to serve on Government commissions relating to decimal coinage, the sale of gas, the gauge of railways, the national standards of length and weight, harbours, lighthouses, sewers, and such things, any or all of which subjects might have been dealt with and expanded to form a readable history of the progress of domestic science with which Airy was connected, for which chapters space might have been found by sacrificing many of the private details above referred to. But this was no doubt foreign to the Editor's purpose, who possibly set himself the task of merely publishing the papers which his father had left as his autobiography; and this being so we can have no quarrel with him.

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* Autobiography of Sir George Biddell Airy, K.C.B., M.A., LL.D., D.C.L., F.R.S., F.R.A.S. Edited by Wilfrid Airy, B.A., M.Inst.C.E. Cambridge, 1896.

There is little indication in the book as to Airy's character, beyond his taste for domestic happiness, his love of order, of which he regarded the science of pure mathematics as simply an extreme case, and his disregard of social honours and distinctions (it was only at a third offer that he accepted knighthood). We are told that as a boy at school he was not popular, and this negative might be applied to him throughout his life. He had a reputation for moroseness. His initials have been said to represent "Great Bear Airy," the first two words not being taken astronomically. He had some friends, chiefly men of scientific tastes; but these were few, as he would only treat with cordiality those whom he thought approached himself in point of intellect; and even from these he would brook no opposition, as may be judged from his quarrel with Sabine, referred to on p. 248 of this book, who had ventured to call in question the efficiency of the Magnetic Observatory at Greenwich. Of the unfortunate incidents relating to the discovery of Neptune, which have made the name of Airy not a little disliked in Cambridge, little is said; there is a very brief account of some facts in this connection in his diary for 1845, to which is appended the remark—written, we should fancy, with a grim chuckle—"I was abused most savagely both by the English and French." After writing the previous sentence we feel that we have done the book an injustice by saying that it does not reveal his character, for indeed there are touches here and there which show the self-satisfaction of Airy which was born of his strength of mind; for he was a strong man and an energetic one, as will be realized by anyone who reads this book.

THE CAPE PHOTOGRAPHIC DURCHMUSTERUNG.—In 1882 Dr. Gill was so much struck by the sharp definition and large number of stars shown on some photographs of Finlay's Comet, taken with rough-and-ready apparatus, as to decide on executing by photographic methods a scheme which he had in mind of continuing Schönfeld's Durchmusterung down to the S. Pole, and thus forming for the Southern Hemisphere a catalogue similar to the Bonn Durchmusterung. He employed for this purpose a rapid rectilinear Dallmeyer lens of 6 inches aperture and 54 inches focal length; each plate covered 6° by 6° of the sky, and plates were taken with their centres 5° apart on parallels of declination 5° apart. Different kinds of plates were used in the course of the work; the exposures given were at first an hour, but with the increase of sensitiveness of plates this was diminished to half an hour: the time of exposure being determined by the condition that every star given in a selected area of Argelander's Durchmusterung should be shown on the photographic plates. All areas were taken in duplicate to eliminate the risk of setting down an accidental speck as a star.

The laborious task of measuring these plates was undertaken by Dr. Kapteyn. He devised a measuring apparatus which gave the Right Ascensions and Declinations directly with which the positions and diameters of about 300 or 400 star-images an hour could be obtained by two measurers with a clerk to enter the results in ledgers prepared for the purpose. The measures were made in duplicate, and discordances, omissions, &c. were all examined by Dr. Kapteyn himself.

The first instalment of Dr. Gill's and Dr. Kapteyn's labours is a quarto volume containing, besides two introductions, 650 pages, each page containing about 240 stars, and extends from declination $-18°$ to $-37°$; two other volumes will be required to complete the work. Comparison with various catalogues shows that the probable error of the Right Ascensions is about $\pm 0^{s}{\cdot}26$ and of the Declinations about $\pm 2''{\cdot}7$. Besides the Right Ascension and Declination for 1875·0, the photographic magnitude of each star based on the measured diameters and deduced according to a discussion by Dr. Kapteyn in the introduction. In addition, references are given as far as practicable—it not being convenient to print more than five—to the catalogues of precision in which the star is given, along with its visual magnitude: it being Dr. Kapteyn's aim to give as far as possible "a condensed history of the observations made on every individual star of the catalogue."

Dr. Kapteyn's introduction contains a discussion on photographic magnitudes. The formula he uses is

$$\text{magnitude} = \frac{\text{B}}{\text{diameter} + \text{C}},$$

where B and C are obtained independently for each plate. This formula is, of course, empirical, but suited the observations best of those suggested, and the constants are deduced by comparison with the magnitudes (visual) of Schönfeld's Durchmusterung or Gould's 'Zone Catalogue.' In this way the mean photographic magnitude is made to agree with the mean visual magnitude; and differences arising from the plates, the exposures, the state of the sky will be shown in the different values of B and C.

In the discussion of the magnitudes it is found that in some parts of the sky many more stars are shown on the plates than are given in Gould's or Schönfeld's catalogues, and Dr. Schönfeld concludes that "stars in the Milky Way are in general bluer than stars in other parts of the sky."

Although we hope to see Dr. Gill's and Dr. Kapteyn's work to some extent superseded by the International Chart of the Heavens, it is not very likely that the measures and reductions for the Southern Hemisphere will be completed in less than twenty-five years, and till then, at any rate, the Cape Durchmusterung will be of great value. More than this, it will render a very great service to those engaged in the Photographic Chart of the Heavens, both in the identification of the stars and in the examination of individual plates, to see that stars down to the desired limits of magnitude are shown, and will also serve as a general check of the reductions.

In respect of the printing and general arrangement the Cape Durchmusterung is all that can be desired, and may well cause astronomers in the Northern Hemisphere to wish the Bonn Durchmusterung like it. F. W. D.

NOTES.

Comet Notes.—The following elements of Perrine's Comet (*f* 1896) have been deduced by Prof. Knopf from observations on November 4, 6, 12 (Ast. Nach. 3387):—

$T =$ 1897 Feb. $8^{d}\cdot13$ Berlin M.T.
ω .... 172° 9′
☊ 86 24
i 146 9
q .... 1·0647

The comet was observed at Greenwich on November 29 and 30, being then fairly bright, and readily visible with an aperture of 6½ inches. It had a decided nucleus. The place agreed closely with that deduced from the above elements; these are, therefore, probably nearly correct.

The following ephemeris is for Berlin midnight:—

	R.A. h	m	s	S. Dec. °	′
Jan. 4....	19	48	32	7	2
8....	19	48	33	8	19
Jan. 12....	19	48	37	9	35

After this the comet is lost in the Sun's rays, and on its reappearance it is only visible in the southern hemisphere. We have computed the following extended ephemeris for the benefit of southern observers. This is for Greenwich midnight.

The brightness reaches a maximum on April 7, when it is 6·1 times that at discovery. On March 4 and April 29 it is 3·0 and 2·9 respectively.

The comet is nearest to the Earth on April 12, when its distance is 73,000,000 miles. The comet is certain to be a conspicuous telescopic object in March and April, and may possibly be visible to the naked eye.

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	'		h	m	s	°	'
Feb. 8....	19	48	28	18	46	Apr. 7....	17	14	31	75	16
16....	19	48	0	22	5	9....	16	2	44	77	49
24....	19	47	4	26	0	11....	14	26	8	78	46
Mar. 4....	19	45	12	30	54	13....	12	50	52	77	37
12....	19	41	22	37	12	15....	11	42	4	74	49
20....	19	32	56	45	42	17....	10	58	16	71	10
24....	19	25	8	51	3	21....	10	11	22	63	21
28....	19	12	8	57	17	25....	9	49	2	55	55
Apr. 1....	18	48	42	64	21	29....	9	37	6	49	23
5....	18	0	12	71	49						

Ast. Nach. 3391 contains new elliptical elements of Giacobini's comet, by Messrs. Hussey and Perrine, deduced from numerous observations extending from Sept. 8 to Oct. 29.

T = 1896, Oct. $25^{d}\cdot985$ Greenwich M.T.
ω .... $139^{\circ}\ 29'$
☊ 192 6
i 11 33
ϕ 41 6
log a...... 0·6360
Period .... $9^{y}\cdot00$

The following ephemeris for Berlin midnight has been deduced by Prof. Kreutz from Perrotin's elements (Ast. Nach. 3386). The R.A. of the ephemeris is probably too great by some 50 seconds:—

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	'		h	m	s	°	'
Jan. 1....	23	3	58	6	43	Jan. 13....	23	38	37	4	15
5....	23	15	43	5	54	17....	23	49	46	3	24
9....	23	27	16	5	5	21....	0	0	43	2	34

The brightness is steadily diminishing, and on January 5 is 0·39 of that at discovery.

The following ephemeris of Brooks' Comet (1889 V.) is for Berlin midnight (Ast. Nach. 3389). The brightness is now only half that at re-discovery:—

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	'		h	m	s	°	'
Jan. 2....	23	54	29	0	27	Jan. 20....	0	27	41	4	42
8....	0	5	22	1	52	26....	0	39	5	6	6
14....	0	16	27	3	17	Feb. 1....	0	50	36	7	30

A new comet (g 1896) was discovered by Perrine on December 8 The comet is stated to be bright, with a well-defined nucleus, and a tail some 30' long. It is well placed for observation, being on the meridian at about 8 P.M.

The following elements and ephemeris for Greenwich midnight have been deduced by Ristenpart from observations on December 8, 9, 10:—

$$\begin{array}{lll} T = 1896 \text{ Nov. } 30^{\mathrm{d}}{\cdot}71 \text{ Greenwich M.T.} & & \\ \omega \;\ldots\ldots\ldots & 170^{\circ} & 3' \\ \Omega \;\ldots\ldots\ldots & 244 & 50 \\ i \;\ldots\ldots\ldots & 14 & 46 \\ q \;\ldots\ldots\ldots & 1{\cdot}1481 & \end{array}$$

	R.A. h m s	S. Dec. ° ′		R.A. h m s	S. Dec. ° ′
Jan. 3....	3 38 48	1 4	Jan. 15....	4 31 26	1 29
7....	3 57 56	1 22	19....	4 45 52	1 21
11....	4 15 36	1 31	23....	4 58 41	1 5

The brightness is decreasing, being on January 3rd 0·46 of that at discovery.

Ast. Nach. 3389 contains definitive elements of Comet 1890 I. by Frank G. Radelfinger; this comet was discovered by Borrelly on December 12, 1889, and 78 observations are available, the last being on 1890, January 16.

$$\begin{array}{l} T = 1890, \text{ Jan. } 26^{\mathrm{d}}{\cdot}5178 \text{ Berlin M.T.} \\ \left.\begin{array}{lrrr} \omega \;\ldots\ldots & 199^{\circ} & 51' & 40'' \\ \Omega \;\ldots\ldots & 8 & 28 & 6 \\ i \;\ldots\ldots & 56 & 44 & 15 \end{array}\right\} 1890{\cdot}0 \\ \log q \;\ldots\; 9{\cdot}43098 \end{array}$$

A. C. D. C.

Minor Planet Notes.—The following planets, discovered by Charlois, 1892, November 25, 28, have been named (Ast. Nach. 3389):—(346) Hermentaria; (347) Pariana.

A new planet, DB, was discovered by Charlois on 1896, December 7. He discovered another, DC, on Dec. 28.

An Appendix to the 'Berliner Jahrbuch' has been issued containing ephemerides of a great number of planets which pass opposition during the next six months. A. C. D. C.

OBITUARY: DR. B. A. GOULD.—BENJAMIN APTHORP GOULD was born in Boston, September 27, 1824. He entered the Boston Latin School in 1836, and graduated from Harvard College, with high distinction in classical, as well as in mathematical and physical studies, in 1844. After teaching for a year in the Latin School, he decided to devote himself to a purely scientific career. As a preparation for this, he went to Europe in July, 1845, to study astronomy, working at the Greenwich Observatory three months, at the Paris Observatory four months, at the Berlin Observatory a year, at Altona Observatory four months, and at Gotha Observatory one month. He returned home in 1848. In 1852 he was appointed to take charge of the longitude-determinations of the

Coast Survey. He organized, developed, and extended this service, retiring in 1867. Meanwhile, in 1855, he became Director of the Dudley Observatory in Albany, equipped and organized the institution, and carried it on without remuneration and at his private expense. He left it in 1859, after a severe struggle to preserve the institution for purposes of scientific investigation.

In 1859 he published his discussion of the places and proper motions of circumpolar stars, for use as standards in the Coast Survey. These, as revised by him in 1861, together with his similar list of clock-stars, were adopted as the standards for the American Ephemeris, and, as to the circumpolars, remain in such use to this day. In 1866 he published his reduction of D'Agelet's observations. About the same time he performed a similar service for the greater part of the observations made at the United States Naval Observatory since its establishment, and also for the expedition to Chili to determine the solar parallax. In 1866 he planned and executed the work of establishing, by the Atlantic cable, the relation in longitude between European and American stations. As actuary of the United States Sanitary Commission he conducted, and published in a large volume, extensive and important researches upon Military and Anthropological Statistics and the Distribution of Population. About the same time he undertook the reduction of Rutherfurd's photographs of the Pleiades. The results, partially published in 1866, were submitted completely, in an elaborate memoir, to the National Academy in 1870, together with a second memoir on the Praesepe. He was, indeed, a pioneer in the utilization of photography for exact astronomical measurement. About 1864 he built an observatory in Cambridge, equipped with an 8-ft. transit-instrument, and, until 1867, carried on a determination of the right-ascensions of all the stars to the tenth magnitude within one degree of the pole. This work was completely reduced, but the discussion and publication were postponed by his removal to Cordoba.

In 1865 he became intensely impressed with a desire to explore the southern celestial hemisphere. The opportunity to do so soon came. This project assumed at first the form of a private astronomical expedition, for which his friends in Boston had promised the pecuniary means; but, under the enthusiastic support of Mr. Sarmiento, at first as Argentine Minister to this country, and later as President of that Republic, it rapidly broadened, and finally lead to the establishment by Dr. Gould of a permanent National Observatory at Cordoba. This marks an epoch in modern astronomy, the equalization of our knowledge of the two celestial hemispheres. The institution and its work form an impressive monument to his memory.

It is impossible, in brief space, to describe or characterize the marvellous work here undertaken and so faultlessly pushed to completion by Dr. Gould, during the fifteen years of self-imposed exile from his native land, with unfaltering devotion and energy, in the face of difficulty and domestic bereavement. The work on the uranography of the southern heavens was finished in 1874, and was published under the title of the "Uranometria Argentina," which will remain a classic for all time. The zone-observations, of the stars between 23° and 80° south declination, which were the original and always the dominant object of the enterprise, were begun in 1872, substantially completed in 1877, and revised in 1882–83. This work was embodied in the Zone-Catalogues containing 73,160 stars, which appeared in 1884. Parallel with this, and almost overshadowing it in importance, was carried on the independent series of meridian-circle observations for the General Catalogue of 32,448 stars, completed in 1885. Dr. Gould, besides, left the manuscript of the remainder of his series of fifteen volumes, not then published—containing the observations and the annual catalogues, incorporated in the General Catalogue—complete to the minutest detail, ready for the printer. These have since appeared from time to time—the last volume, rounding out his work, reaching Cambridge but a few hours before his death.

Another part of the work for the Cordoba Observatory, planned by Dr. Gould as a fitting extension of it, was a Durchmusterung of the southern sky. For this, indeed, he had provided the instrumental means and trained the assistants, it being his purpose to be ready to begin it at any time in case of unforeseen delay or accident to the other work. On leaving Cordoba he confided it to the care of Dr. Thome and Mr. Tucker, who have since so worthily conducted it.

Dr. Gould was one of the earliest to recognize the capabilities of photography to render service to astronomy of precision. At Cordoba he secured about 1400 plates showing photographs of southern stellar clusters. The principal occupation of the last ten years of his life has been the measurement and reduction of these.

The establishment of the 'Astronomical Journal' is not the least of the services rendered to the science by the late Dr. Gould. Feeling this to be one of the wants of astronomy, in 1849 he began this journal as a medium for the publication exclusively of original investigations. He edited the journal until at the end of the sixth volume in 1861 its issue was interrupted first by the war for the preservation of the Union, and afterwards by his absence in Cordoba. In 1885 he resumed its publication, which he continued until the end of his life.

Dr. Gould married, in 1861, Mary Apthorp Quincey, daughter of the Hon. Josiah Quincey, and to her his 'Catalogue of Southern Stars' is dedicated. (Abridged from Ast. Jour. No. 389.)

NATURE for Dec. 24 announces the death of Mr. Sidney Waters, a well-known member of the Royal Astronomical Society. He was elected a Fellow in 1873, and shortly after communicated papers on the distribution of star-clusters and nebulæ in space. He has recently continued this investigation by constructing charts showing this distribution in relation to the Milky Way, which were exhibited at the Royal Society's conversazione in 1895. Mr. Waters was a comparatively young man, and his death will come as a shock and surprise to many.

The death is also announced of Dr. Axel Möller, Professor of Astronomy in the University of Lund, and Director of the Observatory. The work for which he will be chiefly remembered is his investigation of the motion of Faye's Comet, and the calculation of the disturbance of its orbit by the planet Pandora, for which he received the Gold Medal of the Royal Astronomical Society in February 1881. As Director of the Lund Observatory, he has contributed the zone +35° to +40° to the Catalogue of the Astronomische Gesellschaft. Dr. Möller was elected an Associate of the R.A.S. on 1874 Nov. 13. He was born 1830 Feb. 13, and was therefore in his 67th year at the time of his death.

The Companion to Sirius.—It will be remembered that, on Aug. 31, Dr. See discovered and measured a faint object which he thought was the companion of Sirius. Dr. See observed with the 26-inch Lowell refractor. As Messrs. Lowell and Cogshall also measured this object, and saw it practically in the same position, the discrepancy between the observed and predicted place seemed to point to some defect in the calculated orbit. Since then, however, the Lick observers have also measured what they take to be the companion, their place agreeing well with the predicted. The measures are:—

1896·671....	224°·9	6″·32	Cogshall.
·672....	225 ·2	5 ·28	Lowell.
·678....	220 ·6	5 ·07	See.
1896·819....	189 ·3	3 ·68	Aitkin.
·829....	189 ·8	3 ·65	Schaeberle.

Does this mean that there are two companions?

The Companion to Procyon.—With reference to this companion, which, as we announced in our last number, has recently been re-discovered by Prof. Schaeberle at Lick, it may be of interest to recall its previous history. Dr. Auwers, from a discussion of transit observations of Procyon, predicted a companion in position-angle 97°, this position being for March 1874. O. Struve had reasons for suspecting the existence of a companion in 1873, and in 1874 he and his assistant measured one in position-angle 95°; and although the companion could not be seen with the Washington 26-inch, Prof. Struve was confident of its reality. The period determined by Auwers was 39·9 years, radius of orbit 1″·0.

That Procyon has a disturbed motion is borne out by Mr. Thackeray's discussion of the Greenwich Transits for 60 years

(M. N. 1895, Nov.), but for over 20 years no one has seen this companion, and the observers at Lick are to be congratulated on the result of their search.

In 1876, when Messrs. Clark, Watson, Peters, Newcomb, Hall, and Todd were looking for this companion they found three others in positions (10°—6″), (36°—8″·8), and (50°—10″).

STARS HAVING PECULIAR SPECTRA.—Harvard Circular No. 12 gives a list of 9 stars whose spectra have been noted by Mrs. Fleming as peculiar in her examination of the Draper Memorial Photographs. The spectrum of one of these, ζ Puppis, is thus described:—"The continuous system is traversed by three systems of lines. First the hydrogen lines and the line K, which are dark, as in stars of the first type. Second, two bright bands or lines whose approximate wave-lengths are 4652 and 4698, which may be identical with the adjacent lines in spectra of the fifth type. Third, a series of lines whose approximate wave-lengths are 3814, 3857, 3923, 4028, 4203, and 4505, the last line being very faint. These six lines form a rhythmical series like that of hydrogen, and apparently are due to some element not yet found in the stars or on the Earth." The circular goes on to say that the formula of Balmer will not represent this series of lines, but that the numerical values of the wave-length are approximately represented by the formula $4650 \frac{m^2}{m^2 - 4} - 1032$, when m is put successively equal to 10, 9, 8, 7, 6, 5.

Two other of the 9 stars are said to have spectra similar to this, containing two bright bands resembling, and perhaps identical with, those in the spectrum of ζ Puppis.

A METHOD OF DETERMINING RELATIVE MOTION IN THE LINE OF SIGHT.—Harvard College Circular No. 13 describes a method which is being tried by Prof. E. C. Pickering and Mr. E. S. King for using the objective prism to determine the relative motion of two stars in the line of sight, which two must, for the application of the method, be near enough together to be photographed upon the same plate. Let A and B be two such stars, A being at rest and B approaching with such a velocity that a given line in its spectrum is deviated by the amount d, and let a photograph be taken in such a position that the end of shorter wave-length of the spectrum of B is turned towards that of A. Then the distance between the images of the given line in the two spectra will be less, by the amount d, than it would be if both stars were at rest. Now let another photograph be taken in which, by turning the prism 180°, the spectra are turned by the same amount, so that the end of greater wave-length of the spectrum of B is turned towards that of A. The distance between the two lines will then be increased by an equal amount. If two such photographs are superposed and the images of the reference line in the spectra of

A are made to coincide, its images in the spectra of B will deviate by 2*d*. To apply this method, a photograph of a region a little east of the meridian is taken in the usual way. Then the telescope is reversed and a second photograph of the same region on a plate with the film side away from the star, so that the photograph is taken through the glass. As both photographs are taken near the meridian, the lines will be nearly perpendicular to the length of the spectrum, while at a large hour-angle, if the exposure is long and the spectra narrow, the lines will cross them obliquely, owing to the differential refraction. Reversing the telescope turns the prisms, and with them the spectra exactly 180°. In making the examination the plates are placed film to film, so that the spectra are side by side, and one is moved over the other by means of a micrometer-screw.

Some preliminary tests have been made, but no results are given which would show the success of the method, or the reverse.

SUN-SPOTS AND THE WEATHER.—The latest research on this subject is by M. Marcel Brillouin, who communicated a paper to the 'Comptes Rendus' through M. Mascart. His theory is that whenever a spot is brought by the solar rotation on to the visible disk of the Sun, that a change takes place in the terrestrial atmosphere. Generally this change is shown by the formation of cirrus cloud in a low-pressure region, which then moves to a region of high-pressure. M. Brillouin supports this theory by stating that he had observed for more than a year that whenever cirrus of this character had formed, a new spot had entered on the E. limb of the Sun.

A writer in the B. A. A. Journal pertinently remarks on this, that so long as fresh spots are almost of daily occurrence this fact is of little value. It will be well to wait for the sun-spot minimum before placing much trust in this theory.

THE SOLAR ECLIPSE OF 1896, AUG. 9.—M. Deslandres presented his report on the expedition to Yezo to the Academy of Sciences on Dec. 7, which is published in the 'Comptes Rendus' for that date. After describing the personnel, circumstances, and equipment of the expedition, he goes on to say that at the approach of totality clouds did not entirely prevent the crescent Sun from being seen, and also that after second contact it was just possible to see the corona, but under conditions so unfavourable that he resolved to abandon his original programme, and to expose plates in the seven cameras for the whole time of totality ($2^m\ 35^s$) on the eclipsed Sun. Six of the seven exposed plates show an image of the corona more or less distinct. The seventh shows nothing. "The photographs show the coronal ring diffused at its internal edge, and nearly destitute on the exterior of characteristic rays. There can only be seen faint streamers in directions N.W., N.E., and S.W. But these plates show plainly the general distribution of light in the corona: a study of them leads to some general results, and throws light on the question of the connection between the spots and the corona."

THE "GEMINID" METEORS.—Prof. A. Herschel sends us from Slough a note of his recent observations of this meteor-shower; he says:—"The 'Geminid' shower was going on briskly when I returned here at $11^h 50^m$ P.M. from Burlington House last Friday, Dec. 11th, and I mapped 9 tracks between $11^h 55^m$ and $12^h 25^m$ (or at the rate of 18 per hour), when the sky clouded over and remained overcast throughout the night. Last night (Dec. 12), with the sky again quite clear, only 10 or 12 Geminids were mapped from 10 P.M. until midnight, or at the rate of 5 or 6 per hour; showing that the maximum certainly took place this year on the 11th (as it should do owing to leap-year), instead of, as it most frequently does, on the night of Dec. 12.

THE RECENT EARTHQUAKE.—Though this phenomenon scarcely comes under the head of astronomy, it may be well to record here that at about half-past five on the morning of Thursday, Dec. 17, an earth-wave was distinctly observed to pass through England. The wave appears to have originated in Hereford, and to have travelled eastward, just affecting the northern suburbs of London; no great damage was sustained.

TIME-RECKONING.—From May 1 next Belgium will adopt the system of time-reckoning for general use in which the day is divided into twenty-four hours numbered consecutively; the hour after noon being called 13 instead of 1 P.M., as heretofore. It will be remembered that Greenwich Time is the standard legal time for Belgium. We have reason for thinking that the universal or zone time-system may possibly be soon greatly developed. The subject is being actively pursued by the Royal Society of Canada, which body has for a long time taken much interest in it.

WE have been informed that M. Maurice Loewy has been appointed to the Directorship of the Paris National Observatory, rendered vacant by the recent death of M. Tisserand. M. Loewy has been a member for many years of the staff of the Paris Observatory, and previous to his new appointment held the position of *Sous-Directeur*. He has done work in many branches of astronomy, the Equatorial Coudé is his invention, and his lunar photographs, taken in co-operation with M. Puiseux, frequently figure in the list of presents to the Royal Astronomical Society. M. Loewy is a member of the Bureau des Longitudes, and has taken an active part in several longitude determinations. He has published catalogues of stars, besides many investigations on subjects of geometrical astronomy.

A MONUMENT in memory of Father Secchi, the former Director of the Observatory Collegio Romano, has been erected at Reggio (Emilia), where he was born. The sum of 78,000 francs was publicly subscribed for this purpose.

THE Editorship of the 'Astronomical Journal,' left vacant by the death of Dr. Gould, has devolved on Dr. S. C. Chandler, whose frequent contributions have added much to the value of this publication in recent years.

M. POINCARÉ has been appointed Professor of Mathematical Astronomy and Celestial Mechanics in place of the late M. Tisserand.

FROM AN OXFORD NOTE-BOOK.

"I SUPPOSE you will become sane again now?" was the greeting of a friend the other day, *à propos* of the return of the Note-book from its wanderings. It somewhat alarmed me to find that the giddiness caused by performing a half-revolution of the earth in both directions had been so noticeable, and I will do my best to return to a proper sobriety, however difficult this may be. There is a certain seasonableness about this effort too, considering that Christmas is just over: though this latter fact is not conducive to serious and sustained mental effort. But before the relapse I feel compelled to give my gentle reprover a severer shock than usual, and hence I am going to devote one more paragraph to a journey round the world—not half round, but all round this time, and, moreover, "Round the World with a Gaiety Girl."

WHO would suppose that a book with this title "Round the World with a Gaiety Girl" contained anything to interest a sober astronomer? The last three words of the title should really have another set of inverted commas to themselves; for they are the name of a lively play which a wandering company played in all parts of our earth; and the book recounts their adventures. But the heading of the chapter on Australia contains the sub-headings *Sydney*: Beautiful Weather—Public Buildings—Jails—University—Observatory—Suburbs—the Harbour; and so I spent two shillings on the book to see how Mr. Russell had got mixed up with the plot. I was also curious to know whether the proximity of the Observatory to Jails was entirely accidental; whether, perhaps, those who had erred in public buildings and been committed to jail had opened to them a path, through hard work at the University, to the peaceful life of an astronomer, terminating in a retirement to the suburbs. But the next item in the programme was startling: that any author should have separated Sydney and its harbour so widely shook one's faith in him to its foundations, and without further speculation I turned to the actual contents. They were not very exciting. It will be best to give the whole astronomical extract as it stands, to save others the reference:—"Some of the other buildings we visited also, but memory unfortunately fails to recall their attractions. To the Observatory, a great improvement on the older establishment out Parramatta way, we were most hospitably welcomed by Mr. H. C. Russell, the Government Astronomer, who brought us nearer to that disappointing constellation, the Southern Cross, and showed us some of his beautiful sky photographs." This brief account is all very well in its way, but does not represent the author at his best. Witness the following vivid sentence descriptive of a troubled crossing of the Atlantic:—"On the sixth day from port, one of the company was heard asking loudly for the bathroom, to which a benevolent steward kindly conducted him."

THE mention of the Sydney Observatory reminds me that Mr. Russell has lately passed through rather a trying ordeal with triumph. A "Civil Service Board" was appointed by the Parliament of New South Wales to reorganize the Civil Service (which of course includes the Observatory), with the object of reducing expenditure in every possible way. The Board managed to reduce the Observatory vote by the large sum of ten pounds! This says a great deal for the economical organization of the Observatory, and Mr. Russell must feel greatly relieved, although it is bad enough to lose even ten pounds in these times.

THE return of the Notes to their ordinary course is a suitable occasion for making a recantation. Before leaving for Japan I had accused Mr. Percival Lowell of a mathematical error in his book on Mars (Obs. 1896, April, p. 178). Mr. Lowell wrote supporting his original statement (p. 198), and he was, as a matter of fact, quite right, and my criticism wrong. For the long delay in making this acknowledgment, I have only the excuse that Mr. Lowell himself puzzled me by writing a letter curiously at variance with his paper; and being very busy at the time with preparations for the eclipse which would not wait, all I could do was to print his paper (as on p. 198), and to give the ambiguous sentence from his letter on p. 217. But when leisure allowed of a careful reconsideration of the matter, I saw that the sentence in the letter must not be taken too seriously, while the paper must. I may say that I wrote to Mr. Lowell to this effect some time ago.

A REVIEW of the lately published Autobiography of Sir George Airy in the 'Pall Mall Gazette' of Nov. 24th last introduced to us several new facts, of which the following extract is a sample:—

"**Another controversy in which Airy became engaged relates to the rival claims of Adams and Le Verrier as discoverers of Neptune. The story is an old one,** how Adams, working out his great theory with the aid of a pocket telescope, sent to Greenwich the calculations embodying his work, and how they were there pigeon-holed and ignored until the French astronomer had published his version.

Read between the lines this emphasizes a remarkable feature of the Autobiography, viz. that the references to the discovery of Neptune are so slight—a mere sentence or two. The poor reviewer, after hunting up and down the book for information, was obliged to fill in the "old story" from memory and imagination, and hence the unfortunate "pocket telescope." The Autobiography is in fact a faithful reflection of the diary kept by Airy, in which there is no mention of the matter at all in 1845, although careful records of the weather are made. Airy's wonderful method was in fact sometimes a weakness rather than a strength, as Mr. Wilfrid Airy, in editing the Autobiography, has himself remarked. When the weight of years began to tell, he became almost more anxious to file or pigeon-hole his letters in their proper places than to master their contents or answer them. And without laying too much stress on this mastery of the ruling passion at the close of a long life, we may perhaps divine from it something of the strength of this passion for order and method throughout Airy's working life. The weather clearly lent itself readily to methodical record; while to pick out from the events and correspondence of the day any prominent or exceptional feature would involve a conscious and critical effort. Hence the visit of Adams to the Observatory in 1845 October was not recorded, though the weather was.

But this is not the place or the time to go over the "old story" again, though much might, and probably will still, be written on the subject; and especially on the point that it was Airy's method which lost England the discovery of Neptune. Personally I think he has had far more than his share of blame: he was "savagely abused both by French and English," when that abuse might have been more justly bestowed elsewhere. But the whole story is told with admirable fairness by Dr. Glaisher in the introduction to the 'Memoirs of Adams,' of which the first volume has just appeared. Fortunately, as Dr. Glaisher remarks, there is no dispute about any of the facts: only about the inferences to be drawn from them. These inferences each generation will doubtless colour to its own liking.

DR. MAX WOLF gave us the pleasure of seeing him at Oxford recently. He brought some of his wonderful long-exposure photographs to show us, and beautiful they are. One could not but feel more interested in those on which minor planets were shown, especially with Dr. Wolf as commentator. He pointed out a little faint streak on one plate as a minor planet, and after looking at the region closely I exclaimed that there was another similar streak not far off. "Ah! you think you have made a discovery," he said, "but no! not on that plate: it has been too carefully examined."

The streak I had noticed was an accidental defect, according to the evidence of another plate exposed at nearly the same time. No discovery is announced unless confirmed by another plate, the method of work being to expose a plate for 2 hours, and at the middle of this exposure to begin another plate of the same region, which is carried on an hour after the first is finished. Thus the region is watched for 3 consecutive hours by the two plates, the middle hour being represented on both plates. It will be easily seen how complete a check this affords on discoveries. It is gratifying to learn that the fame of Dr. Wolf's work has secured him a new observatory and a much finer telescope—a photographic doublet of 16 inches aperture.

It is my privilege to follow the course of Dr. Murray's work on his big dictionary by dint of receiving occasional queries from him on astronomical subjects. He must be getting near the end of D by this time, for the last two were about the dumb-bell nebula and dynamometer. He enquired when the former name was first bestowed, and whether the forms dynameter or dynamometer were used indifferently. I found it rather difficult to answer these questions satisfactorily, though very interesting to try. Herschel's original paper in Phil. Trans. 1833 uses several terms to describe the dumb-bell nebula, without conferring this distinctive name upon it; and references to dumb-bell nebula and to dynameter or dynamometer in the 'Monthly Notices' are very few.

In this magazine, and in others with an astronomical basis, it is occasionally the melancholy duty of the editor to record losses by death; but as yet there has been little or nothing said of occasions for rejoicing, which our daily contemporaries place in juxtaposition to the obituaries. I don't know why we should not follow the good example. I am sure we all rejoiced a month or two ago (all of us who heard of it) at an event which united the names of Delaunay and Cornu; and the other day I was glad to hear of the marriage of Mr. Gerrish, of Harvard University Observatory, who looks after so many ingenious mechanical devices there. The difficulty about such an enterprise as that of recording the marriages of astronomers is chiefly, I suppose, that of making it complete. Some people are shy of letting such things be known; but the Editor of this magazine assures me that he will gladly print any news of the kind, if anyone will be good enough to send it him.

The following extract from the 'Birmingham Daily Post' of December 28 reads at first sight as though the number of minor planets was approaching two thousand:—"The name of Berlin is now immortalised in the heavens, for the astronomer Witt has christened the asteroid 1896, which he discovered, Père Berolina."

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 250. FEBRUARY. 1897.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, January 8, 1897.

Dr. A. A. COMMON, F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. TURNER, M.A., B.Sc., and E. W. MAUNDER.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Maunder. Eighty presents have been received since the date of the last Meeting. Among these is vol. v., part 5, of the Astronomical Papers issued by the American Nautical Almanac Office, which contains papers by Prof. Newcomb on the Mass of Jupiter, and another on the Orbit of Polyhymnia. Also a catalogue of 16,748 southern stars deduced from observations by Lieut. Gilliss, in his well-known expedition, presented by the U.S. Naval Observatory. The 'Nautical Almanac' for 1900, presented by the Lords Commissioners of the Admiralty. Mr. J. C. Clancey, who is at present a Candidate for Fellowship of this Society, has presented five works on Land-Surveying, of which he is the author; the Italian Government have kindly sent us the sixth volume of the edition of the works of Galileo, which they are now preparing. We have also received from Prof. K. D. Naegamvala some photographic reproductions of a photograph of the nebula M 8 Sagittarii, and from Mr. J. Lunt a photograph of the partial phase of the solar eclipse of 1896, Aug. 9, taken at Vadsö.

Rev. A. L. Cortie read a paper on "A Method of determining the Approximate Heliographic Latitude and Longitude of Sun-spots and Faculæ on Drawings of the Sun." A set of eight diagrams had been prepared on transparent paper, each showing the limb of the Sun represented as a circle of approximately 10 inches diameter, and having solar meridians of longitude and circles of latitude drawn upon them. These diagrams were drawn consistently with eight different angles that the axis of the Sun makes with the celestial equator; for inclinations other than those for which the diagrams are drawn, interpolation is necessary. Mr. Cortie had used these transparent diagrams to read off the latitudes and longitudes of spots shown on drawings of the Sun made at Stonyhurst, and compared his results with the positions of the same spots as shown in the Greenwich volumes. The comparison showed that the determined longitudes were almost identical, but that the latitudes were not so accordant, there being differences in some cases amounting to half a degree.

Mr. Maunder. Some years ago Dr. Thomson brought out a series of skeleton charts showing orthographical projections of the circles of latitude and longitude on the Sun as seen from the Earth when in different relative positions to the Sun. These were made to the scale in which a solar diameter would be 8 inches, and I used these charts to measure the positions of the spots on a number of photographs, and on comparing these results with those given by the more accurate measurement and reduction of the same spots made at Greenwich, I found that the projection results were correct to about half a degree. I think that from this it is evident that Mr. Cortie's projections must be more accurately drawn than Dr. Thomson's, and that his drawings must be extremely carefully done for the average difference to be so small as that which he has shown to be the result of the comparison. It is clear that for a rough estimation of the positions of spots the projection method is very easy and very fairly accurate.

I have here two photographs of the great spot which has just come upon the Sun, which it may interest the Society to see. The first photograph was taken on January 6, when the definition was very bad, and the picture shows very little more than the immense size of the group. That on Jan. 7 is much better. Although the maximum is now more than 3 years past, this particular group compares favourably as to size with all but two or three which we saw at the very height of the maximum. Its total area is about 2800 million square miles; the length is about 16 degrees of solar longitude.

The President. What is about the size of the Earth as compared with this spot?

Mr. Maunder. The diameter of the Earth is about one-sixth of the diameter of the great leading spot of the group. We were only able to get one photograph on Jan. 7, but the definition is better. The most remarkable feature shown on this day is the intense blackness of the nucleus and its large area. It is unusual to see so large an umbra as there is in this spot. I am hoping that the passage of this spot across the meridian may be accompanied by great magnetic disturbance, because it is fairly compact in form. Extended streams do not give so much disturbance. Probably it will depend upon whether the spot, which has begun to break up, reaches the central meridian sufficiently compact. I may say that about the time it came on the east limb there was a very considerable oscillation of the magnets.

Mr. Newbegin. When does it come on the central meridian?

Mr. Maunder. The entire length of the spot takes more than a day to pass any point, but, roughly speaking, it will be from noon on Saturday to noon on Sunday.

Mr. Maw. Can Mr. Maunder tell us how this group compares as to size with the great group in Feb. 1892? I saw the spot last year, and it struck me as an extensive group, and I am interested in seeing this photograph to-night.

Mr. Maunder. It is almost precisely four-fifths of the area of that of Feb. 1892 as it appeared on the day of maximum, Feb. 10.

Mr. Maw. How long had you begun to observe it before it developed to that size?

Mr. Maunder. We only saw it on Wednesday. We had had cloudy weather before that, so we did not see any sign of it.

Mr. Maw. Had you seen it in the previous rotation?

Mr. Maunder. I have not looked for it; if it was there it must have been very much smaller. It is easily visible to the naked eye.

Prof. Turner. I wish to express the very great interest I take in Father Cortie's attempts to reduce numerical labour. I think figures have been regarded as cheap so long that we have been using them rather too freely in astronomy. I would urge him strongly to continue his investigation because I do not think it would be just to assume from a single set of experiments, such as he has described, that he can always ensure accuracy. The methods used at Greenwich were adopted after very careful consideration and after a great deal of talk, and a debt of gratitude is due to the present Astronomer Royal for having organized this method of measuring Sun-spots, which no one had taken up at all at the time. Personally I can testify to the amount of trouble he took to make this method exact. About the time I was appointed Chief Assistant at Greenwich one of the things on his mind was the question as to whether these methods were accurate or not. I hope that Father Cortie will improve upon them and adopt some method of saving labour, because there is a great deal of labour involved in measuring these photographs. One remark by Father Cortie struck me, namely, that the method at Greenwich was about the most perfect in use. I do not want to criticize the grammar, but I think that a proper description would be that most figures are employed upon it, and possibly perfection is not to be attained by employing as many figures as possible. Again, it seems to me that he sometimes uses two decimal places and sometimes one. I would urge upon him that he should select the decimal place which he will adopt as his criterion of accuracy, and retain it throughout uniformly. This is one of the points which professional astronomers are always urging upon amateurs. I believe mistakes are liable to be made for want of uniformity of system.

The President. It will be remembered that at the last Meeting, when we heard Sir George Baden-Powell's account of his successful expedition to Nova Zemlya, I mentioned that possibly at this

Meeting we might have some account of the less successful, but perhaps as interesting, expedition to Japan, which was undertaken by the Astronomer Royal and others, and also of that to Vadsö, whither I went with a numerous party. Prof. Turner will give you a short account of the Japan Expedition, and I shall call upon Mr. Hinks to show a few slides which will give you some idea of the chief points of interest in our expedition.

Prof. Turner. I happened to be in possession of a slide—the only slide that we have brought back from our expedition– to illustrate this paper, which is a joint paper of the Astronomer Royal, Capt. Hills, and myself. This slide is the production of a Japanese photographer in Yokohama. A gentleman representing 'Harper's Magazine' photographed us when the Sun was very bright, and we had a slide made by a Japanese photographer, which you now see on the screen. This photograph was taken the day before the eclipse. Saturday was fine, and Monday was very fine, but Sunday was not favourable for seeing the eclipse, for it was very cloudy. We were told that the chances were very much against us, but from the actual experience of three weeks on the spot, we did not think that it was more than an even chance whether we should see the eclipse or not, and a day or two before the eclipse the chances of fine weather were good. This really shows what amount of trust is to be placed in deductions from meteorological observations. The choice of a station is determined by previously-observed meteorological conditions, and if you make what appears to be a favourable choice, one always seems to be contradicted by the events at the time. When we got to Yokohama, it was reported in the papers that it had been fine at Kushiro and Shibetcha, but these were exactly the two places that meteorological reports warned us against. In the report we have put down the exact figures taken out by the Japanese meteorologists from their records before we got there. I believe Prof. Todd had asked them to collect records of 3 previous years as data to select the probably best station, and they got out a lot of figures showing that these two places which we were warned against were likely to be unfavourable. This paper is merely an account of what we did in getting ready to which the Astronomer Royal and Capt. Hills referred briefly at the November Meeting. We made out that it was possible to focus the telescopes with great accuracy on the mirror without resorting to star observations. I did get some star observations myself. Then there is a small note on the polariscopic observations.

Mr. Hinks then showed on the screen some slides to illustrate the President's report on the Vadsö expedition.

The President. From the excellent way in which Mr. Hinks has on the spur of the moment given a description of these photographs you may judge what an admirable man he was to have on the expedition. On our voyage I was up every morning for a week at 3 o'clock on the deck of the 'Norse King,' and at this hour the sky was perfect all the way to Vadsö, but when we got within a short distance of Bodö it did strike me that we ought to divide our forces, and that it would be a good thing to leave somebody behind there. Had we followed the principle laid down by Prof. Turner, namely, to disregard meteorological observations, one of us should have stopped there. However, no one did stay at Bodö, although Mr. Hinks offered to do so if called upon. We had made arrangements before leaving London and we had then come to the conclusion that, owing to the low altitude of the Sun at Bodö, it would be better not to occupy that place, but to go on to Vadsö, where the meteorological probabilities were more favourable. I have very little to add to the account Mr. Hinks has given—it is extremely difficult to make much of an unsuccessful expedition; but I may say we had a very pleasant time, and that was at least some compensation for the disappointment. I hope, notwithstanding what Prof. Turner says, that the meteorological conditions at the next eclipse will allow us to get some excellent results, and I look forward to that as a consolation for the disappointment we experienced at the last one.

Rev. W. Sidgreaves. I had intended this evening to read a complete paper on the spectrum of β Lyræ. It is, however, not yet quite ready for presentation to the Society, although the bulk of the work has been ready for three or four months. But the President has consented to my giving a description of its contents this evening, as there are very few papers set down for reading.

The data on which my paper is founded consist of, first, the tabulated relative wave-lengths of the more prominent lines from about 90 photographs of the spectrum arranged in the order of their time intervals from the preceding chief minimum of the star's magnitude; and, secondly, a table of mean wave-lengths for each day of the period, along with a map showing the relative positions of the lines on each of the same days. The order of decreasing wave-lengths has been followed throughout; and I much regret that many astro-physicists have recently decided to follow the reverse order of the pianoforte keyboard in the order of the spectrum lines.

The discussion has proved to be a long one. There are many perplexing changes, and exceptions to otherwise general rules. Many hypotheses to account for these have been started, only to be run down. But there is one amongst these which seems to hold together fairly well, and is interesting as offering the sort of confirmation, which can be drawn from the tables, of the conclusion arrived at by Belopolsky and Vogel—viz., that the division of the bright lines is the effect of superposition of their dark companions. A table of curves has been plotted for the bright lines showing the measured wave-lengths of their centres, with reference to dark Hγ at 4340, for each day of the light period. All these curves are of the same general form, having their highest and lowest ordinates at the first and second maxima respectively; and in all, save one, the difference, or extreme change of wave-length, is very great, and the changes at the two minima are very rapid. The exceptional curve showed comparatively small displacements, but enough to prove that, on the supposition of separate stars for different displacements, the origin of the line was not on the dark line star any more than on the bright line star. Its extreme change was from 4476 to 4470. But there is another curve which starts at the middle of the period at λ 4469, the line being found only on the plates belonging to the 8th and subsequent days; and it was observed that if at the time when this line came into view the other at λ 4474 had disappeared, we should have had a single curve between the extremes 4476 and 4461, of precisely the same form as the rest. In this way it becomes probable that the pair of lines seen during the latter half of the period are parts of the same radiation divided by its fellow-absorption, in agreement with Mr. McClean's remark upon the same line in last month's Mon. Not. The position of the true centre of this line for each day would be given very closely by the mean of those of the parts. The curve thus obtained for the last half of the period can then be produced backwards by applying a constant correction to the position of the outside part of the whole broad line; and the result is a curve of comparatively small changes of relative position, in accord with Vogel's conclusion in favour of the smaller relative displacements. The inference is that all the other curves need the same corrections, and that perfect uniformity is to be obtained by bringing these to the form of the exceptional one.

Mr. Maunder. With regard to the point that Father Sidgreaves has raised, I think the evidence (such as we have in our possession at present) indicates that stars with the wide hydrogen lines are stars of low density, and that stars with spectra like that of the Sun are stars with considerably greater density. That shows nothing with regard to their mass—it is a question of density, not mass. It seems to me there is another explanation. The photosphere in stars does not necessarily form at the same relative level in all stars. It seems to me if we could conceive that it formed in the Sirian stars at a lower level with regard to the chromosphere than in the solar stars, we should have a sufficient reason for the greater breadth and prominence of the hydrogen lines. I was extremely pleased to hear Father Sidgreaves's opinion as to the place in which the red end of the spectrum should be drawn, because, like himself, I am a heretic on that point. The 'Astrophysical Journal' has laid down the law that the red end should be to the right, but I think the whole analogy is the opposite way. I showed some slides of the spectrum of the helium here some time ago, and in those slides the red end was put to the left, and this arrangement was criticized; but it was necessary it should be so, because I was dealing with the question of series of lines, and it seems to me we can only deal with a series of lines by beginning with the left end, in the same way as we begin on the left-hand side of the page of a book. There is also the analogy of the piano. I shall be very glad to see a reversal of the decision of the 'Astrophysical Journal.'

Mr. Newall. I think they are re-opening the question.

Mr. McClean. The absorption-lines appear to remain constant in relative position, and the bright emission-lines do the same, while the two systems shift with regard to each other. The amount of shift may be judged by means of the two absorption-lines at wave-lengths 4472 and 4482 respectively. The corresponding bright line at 4472 shifts at least through a range of that extent corresponding to a relative variation of velocity of the two components of the system in the line of sight of 400 miles a second. This is about the accepted amount.

The delineation of the spectrum from left to right in accordance with the increase of wave-lengths was adopted by Kirchhoff, Thalen, and Angström, also by Huggins, Lockyer, Cornu, Duner, Draper, Pickering, Rowland, and others.

Prof. Alex. Herschel. On the subject of the arrangement of spectrum-lines referred to by Mr. Maunder, Mr. McClean, and Father Sidgreaves, I think that for the proper representation of the spectrum-scale the small wave-lengths ought, as in the high keys of a pianoforte, to be placed on the right. With regard to the changeable appearances presented by the hydrogen lines, I think that more experience of them than I possess is evidently needed to explain them, and Father Sidgreaves's descriptions and interpretations of them therefore appeared to me to be very admirably clear and interesting.

Mr. Maunder then showed a photograph of M 8 Sagittarii, which he said was a copy of the drawing by Sir John Herschel in his volume of 'Cape Observations,' together with a series of photographs of the same nebula taken by Prof. K. D. Naegamvala. The original negative was too thin to show much detail except under the most careful examination, but by successive copying the details were brought out very distinctly.

The President. These are extremely interesting photographs, and there is no doubt that it is possible to bring out in detail by photography things that one cannot see with the naked eye. Many years ago I tried producing copies of pictures, and by taking photographic copies on wet collodion the details were brought out in a remarkable way. This is a point well worthy of the attention of those who want to illustrate their papers by photographs on the screen.

The Ordinary Meeting then closed, and the Fellows present resolved themselves into a Special General Meeting of the Society to consider an alteration proposed by the Council, of Bye-laws 69–74, which form Section XVI., and relate to Awarding the Medal, the alteration consisting of the addition of the word "Gold" before the word "Medal."

The object of this alteration (it had been stated in a circular previously distributed to the Fellows) was to make it possible to award a Silver or Bronze Medal, in addition to the Gold Medal. The Council desired now to award a Bronze Medal, at intervals, in accordance with the provisions of the Mrs. Jackson-Gwilt gift.

On the proposal being submitted to the Meeting by the *President*, *Mr. H. Seward* asked whether the proposed alteration would not place the Society in a similar difficulty as that which the alteration itself was intended to remove, for it might happen that just as now the Council wished to award a Bronze Medal, they might at some time wish to award two Gold Medals, or to award the Gold Medal in a way other than that authorized by the Bye-laws.

Rev. W. Sidgreaves objected to the alteration of the Bye-laws, since he objected to the awarding of any distinction other than the Gold Medal. He thought that it would be more expedient to use the Jackson-Gwilt bequest to make grants of money to persons engaged in researches which required pecuniary resources, not always within the command of the person making the research.

On a show of hands being called for, the alteration proposed by the Council was carried unanimously.

The following papers were announced and partly read:—

F. C. Penrose. "A Method of clearing a Lunar Distance."

W. Schur. "Determination of the Diameter and Compression of the Planet Mars, from observations with the Repsold Heliometer of the Royal Observatory, Göttingen."

Isaac Roberts. "On the Comparison of Reflector and Portrait-lens Photographs."

R. T. A. Innes. "Note on the Magnitude of η Argûs, 1896."

S. W. Burnham. "Orbit of 44 Boötis ♅ I. 15 = Sh. 193 $= \Sigma$ 1909."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites made in the year 1896."

Rev. A. L. Cortie. "Heliographic Co-ordinates of Sun-spots and Faculæ on the Stonyhurst Drawings."

Sir R. S. Ball. "Note on Mr. A. Y. G. Campbell's paper on the Variation of Uncanonical Arbitrary Constants, with an application to the Planetary Theory."

H. H. Turner. "On Differential Refraction to Terms of Higher Order than the First."

Radcliffe Observatory, Oxford. "Observations of Comet 1896 I. (Perrine-Lamp)."

G. Carslake Thompson & H. W. L. Tanner. "Notes on Meteors observed at Penarth, Glamorgan, on 1896, November 14."

The following gentlemen were elected Fellows of the Society:—

William Johnston, Saint Nicholas House, Westgate Street, Gloucester; *George Edward Lumsden*, Assistant Secretary of the Province of Ontario, 57 Elm Avenue, Toronto; *Major Percy Alexander MacMahon, R.A., F.R.S.*, Artillery College, Woolwich, and 52 Shaftesbury Avenue, W.; *Louis C. H. Maston*, San Francisco, Cal., U.S.A.; *Beauchamp Prideaux Selby, J.P.*, Pawston, Cornhill-on-Tweed, Northumberland; *J. E. de Villiers*, Government Surveyor, Cape Town; *John Watson*, Norman Villa, Glossop Road, Sheffield.

The following Candidate was proposed for election as Fellow of the Society:—

John Harold Herbert, Civil Engineer, Mapperley Road, Nottingham (proposed by A. A. Common).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The second ordinary Meeting of the seventh Session of the British Astronomical Association was held at University College, Gower Street, on Wednesday, December 30th, 1896, the President, *Mr. N. E. Green, F.R.A.S.*, in the Chair.

Seven new Members were elected, and the names of twelve Candidates for Membership were read and suspended.

Mr. Walter F. Gale, of Sydney, N.S.W., contributed a paper on "The Brightness and Colour of the Planet Mercury," which was read by the Secretary. The writer said that the eastern elongation of September last afforded a favourable opportunity for determining the stellar magnitude of Mercury, Spica and Arcturus being well placed as comparison stars. Mercury reached its greatest brightness about Aug. 29 and was then perceptibly brighter than Arcturus; its colour appeared an orange-yellow, the same as that of Arcturus, but not quite of so intense a tint.

Two papers by *Mr. Francis McDonald*, of Sydney, entitled, "The Zodiacal Light, 1896," and "A Peculiar Atmospheric Phenomenon," were also read by *Mr. Schooling*.

Mr. E. W. Maunder read a paper on "The Level of Sun-spots," an abstract of which will be found on page 93.

Mr. G. M. Seabroke remarked that Mr. Maunder's theory seemed to agree with his own observations, but he was rather at a loss to understand how the dust could aid appreciably in producing the darkness of the spots. He apprehended that an increased thickness of 20 miles in the extent of the dust layer would have a very slight effect. Then assuming there was a slight depression, that depression would be filled up by the reversing layer of gases, which would flow into it and produce the effect of the widening of the lines which was generally seen in a sun-spot. The theory that a spot was a position where non-luminous carbon existed, together, perhaps, with a slight depression to give a thickening of the lines, seemed sufficiently to account for the darkening of the lines without resort to the idea of darkness produced by increased thickness of the dust layer.

The President asked *Mr. Seabroke* if he thought that would be sufficient to account for the elevation of it.

Mr. Seabroke said it was a most intricate subject. The blackness of the spot could certainly be accounted for by the theory of the non-luminosity of the carbon. Putting aside the question of level, wherever the carbon vapour happened to be non-luminous, there they would have the spot effect—either the Wilsonian or the raised effect.

Miss Brown remarked that Prof. Pereira, of the Azores, was constantly seeing what he called black holes in the umbra, or nucleus, as she preferred to term it, of large spots. This seemed difficult to understand if the depressions were only slight.

Miss A. M. Clerke said that according to *W. E. Wilson's* observations on the heat radiation of sun-spots, those nearest the limb seemed to lose much less by the action of the absorbing layer than did the general surface of the disc, hence they gained in radiation compared with the surrounding surface as they approached the limb, just as faculæ, under similar circumstances, gained relatively in light. This fact told in favour of their being raised, although in an inferior degree to faculæ, above the surface of the Sun.

Dr. Johnstone Stoney said he had listened with peculiar interest to Mr. Maunder's paper, because it gave evidence that the best informed astronomers were coming round to that view of the physical constitution of the Sun which he, Dr. Stoney, laid before the Royal Society as long ago as 1867 in a communication printed in the 'Proceedings' of the Society the following year. In his view the photosphere is a layer of clouds of condensed, or partly condensed, vapours of some material, which, like carbon, is, when condensed, or partly condensed, both black and of low atomic weight, and which is able to maintain itself at a temperature low enough for condensation by the enormous radiation outwards which takes place from it. Everywhere inside this photospheric layer, and for a short distance outside, there prevails a higher temperature, so that the photosphere is a layer of minimum temperature. Any upheaval from beneath may cause the regions of higher temperature on either side to meet, and may thus temporarily obliterate the layer of minimum temperature between them. The clouds will thereupon dissolve and we have a spot. In this spot we get a view of the regions beyond, and if there were nothing to interfere with the radiation outwards of the heat and light from the interior parts of the Sun, the spot would appear to us brighter than the photosphere. Dr. Stoney proceeded to explain his theory that even the central parts of the Sun are in a state of vapour, so that the chemical material of which it consists will range itself upwards to different heights, the lighter constituents overlapping those of greater density; and the appearances revealed to us in sun-spots seem to force us to the conclusion that the outermost of these envelopes must have superficial layers by which incident light can be either scattered or reflected, for otherwise the sun-spots would inevitably appear brighter than the photosphere. Dr. Stoney held that there was no physical reason for supposing that the dark spots were otherwise than very shallow. From the physicist's point of view five miles would be more than sufficient provision to make.

Mr. W. H. Wesley remarked that if the general surface of the Sun was a surface where there was condensation of carbon, the penumbra of the spot a surface where there was less condensation, and the umbra where there was still less, they would expect that the one should run into the other by imperceptible degrees, while, as a matter of fact, the penumbra was almost invariably sharply marked off from the general surface of the Sun, and the umbra, again, was just as sharply marked off from the penumbra.

Mr. Lunt thought it was generally agreed that the darkness of the sun-spot was due to the absence of carbon in the incandescent state in that particular part of the photosphere, and he thought it reasonable to suppose that this was due to some kind of ebullition from beneath the sun-spot. A jet of intensely heated substance from beneath would revaporize the carbon and so cause a dark spot, which would therefore be a hotter region than the neighbouring bright photosphere, and the penumbra might be considered as a disturbed area surrounding the central jet in which revaporization of carbon was only partial.

Mr. Holmes said that if the Sun were looked at during a fog or through a dark glass no difference in brightness towards the limb could be detected. If the centre was four times brighter than the limb, they would expect the centre to remain visible when the limb had vanished, but this was not the case.

Mr. C. Thwaites mentioned that he had often seen the photosphere to appear to encroach upon the umbra and even at times entirely to hide it. This seemed to indicate that the umbra was a depression.

Mr. Maunder, in reply, said his main object in writing the paper was to point out that a sun-spot was a very complex structure and that they must not regard it simply from the point of view of its appearance when examined visually. With regard to the black holes referred to by Miss Brown, he simply regarded them as one stage farther than the umbra. With reference to Mr. Seabroke's question as to the small effect of dust in a spot, that would be quite true if they saw the spot fully presented, but if they saw the spot much foreshortened it was clear they would not be looking through twenty miles of dust, but it might be 20,000. His view was that when the spot was full on the disc, the main cause of the darkness was diminished radiation, and that when they got it near

the limb that was largely substituted by increased absorption. In reference to what Dr. Johnstone Stoney had said, there could be no doubt that in the main their present ideas of the construction of the solar photosphere were due to Dr. Stoney's paper of 30 years ago.

Prof. K. D. Naegamvala **contributed a paper on "A Photograph of Nebula M 8 in Sagittarius and a method of demonstrating faint details in photographs of Nebulæ &c."**

Mr. Lunt **exhibited some photographs taken with a 3¼-in. reflector as preparatory work for the recent eclipse; and** ***Mr. Hardy*** **exhibited a photograph of his model of Mare Crisium and read a paper in which he stated that he had come to the conclusion that there was an atmosphere on the Moon confined to the greatest depths.**

Mr. Crommelin **said it seemed difficult to reconcile the results of occultations and total solar eclipses with the presence of an atmosphere of such density as Mr. Hardy's theory would require.**

Mr. T. Gwyn Elger, **director of the Lunar Section, contributed a short paper, accompanied by a diagram of Plato, in which he asked observers to carefully study the floor under as many different phases of illumination as practicable, in order to confirm, if possible, the existence of a rill or rills recorded by Herr Brenner and others.**

ROYAL METEOROLOGICAL SOCIETY.

THE Annual General Meeting of this Society was held on Wednesday, the 20th of January, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. E. Mawley, F.R.H.S.*, President, in the Chair.

The Secretary read the Report of the Council, which showed that the Society had made steady progress during the past year, there being an increase of seventeen in the number of Fellows.

The President then delivered an address on "Shade Temperatures," in which he stated that of all meteorological observations there were none approaching in importance those made of the temperature of the air—generally known as "shade temperature." Indeed, the first question invariably asked in regard to almost any climate was as to its temperature. Mr. Mawley traced the history of the different methods of exposing thermometers since the time that regular observations of the weather had been made in this country. For many years open screens were most favoured by meteorologists, that devised by Mr. J. Glaisher, F.R.S., and the late Astronomer Royal (Sir G. B. Airy) being the pattern principally used. In 1864 Mr. T. Stevenson, C.E., invented an admirable form of closed screen with louvred sides, which was considered preferable to the open type of screen, and has now almost entirely superseded the Glaisher stand. In 1883 the Stevenson screen was considerably improved by a Committee of the Royal Meteorological Society. Mr. Mawley then described his own experiments at Croydon and Berkhampstead as regards this improved screen, known as the Royal Meteorological Society's pattern. He showed that the only two defects which had been attributed to this form of thermometer-exposure were virtually non-existent, and therefore advised its general adoption both in this country and on the Continent. Mr. Mawley had recently made observations in the Stevenson screen, and also in the screens used in France and Germany, and the conclusion he had come to was that the results obtained in the Stevenson screen were not only the nearest to the true air temperatures, but also more likely to be strictly comparable with temperatures taken in a similar screen, but with different surroundings elsewhere.

The Level of Sun-spots.

It is very easy for me to summarize my views upon this question, which seems to be exciting so much attention just at present. It is more difficult to deal clearly and briefly with the details.

In a word, I do not believe that Sun-spots can be strictly said to have a level. That which we see of a Sun-spot is certainly not the whole of what we must describe under that name; and if we could ascribe a definite level to the spot as it presents itself to us visually, we should be ignoring phenomena which make themselves apparent to us in other ways.

But even confining ourselves to spots as we see them directly, or on ordinary photographs, we are, I think, precluded from assigning them to any general level. The apparent darkness of a spot is certainly not chiefly due to the absorption of any overlying veil, whether of solid, liquid, or gaseous matter. There is evidence of some increase of absorption in a spot in the appearance of new dark lines in its spectrum, and the broadening of others, but it is quite clear that the increase of selective absorption in a spot is not nearly equal to that exercised by the gases of the "reversing layer." If we ascribe a depth to this latter of half a second of arc, say 200 miles—surely more than an outside estimate,—then the *additional* depth of absorbing vapour can, I think, scarcely be put higher than 20 miles.

But even if we exceeded this estimate many times over, and I believe it is excessive as it is, we should still be confronted with the fact that Sun-spots were, relatively to their area, exceedingly shallow bodies. It would follow, if absorbing material were the chief cause of the darkness of a spot, that whether that material floated as a cloud above the photosphere, or filled the hollow in it, a spot would always appear much darker when near the limb than when fully presented, and that a large spot would then appear darker than a small one, especially towards its outer edge. The thickness of the cloud, or the depth of the hollow, whichever view we take, cannot be great, and must at present be a matter of conjecture. The surface-diameter of the spot, on the other hand, which at the limb measures the depth of the absorbing material through which we are looking, is easily ascertained, and we know that it is to be reckoned by thousands of miles, up to even one hundred thousand miles or more.

It is sufficient to state the case to dismiss it. Sun-spots at the limb do not give the appearance which they would on this hypothesis. We may take it, then, that the apparent darkness of a spot is due to an actual defect of radiating power. It is apart from my present purpose to discuss the mechanism of the photosphere, about which, however, there is at present very little dispute amongst astronomers. But I think that one may safely say that at present we have no sufficient grounds for asserting that this surface of diminished radiation must necessarily be at the same level in all spots, or even always at the same level in the same spot. More, we cannot lay it down that it may not be sometimes below, sometimes at, sometimes above the general photospheric level of the Sun.

There is no doubt as to the conclusion to which the study of the forms and changes of spots, when seen fully presented, must lead us. As we see them then, it is clear that the photosphere immediately surrounding a spot is higher than the adjacent penumbra, that the penumbra immediately surrounding the umbra is higher than the umbra it touches. For it is the outer brighter structure which invades the inner darker one.

But even this is not decisive as to the question before us. In the search for the Wilsonian effect, a search which has convinced me that we cannot look to such effect for any satisfactory evidence as to Sun-spot level, I have, I think, come across an unmistakable feature. The umbræ of spots near the limb seem not to suffer foreshortening as quickly as the spots themselves. Indeed, in extreme cases they may be seen as apparently actually bridging a much foreshortened spot. I have also the evidence of careful Sun-spot observers, though I cannot recollect having seen such a phenomenon myself in a really regular spot, that the umbra has occasionally been seen to project right across the further penumbra, and to encroach, like a black tooth, on the photosphere beyond.

This appears to me evidence that the umbra is itself not level, but highest towards its centre. I regard it as having, taken typically, the contour of a very flat dome. The variation in the appearance of different spots proves, I think conclusively, that the angle of elevation of the summit of the dome is subject to wide variation.

But if the umbra has typically this contour as of a shallow dome, I think that both penumbra and surrounding photosphere have a tendency to conform to it. The penumbra is so often brighter at its inner edge that we naturally think of this as its highest region. The photosphere round a large spot is so often brighter than the average, irrespective of the presence of distinct faculæ, that it too must be regarded as rising there above its ordinary level. And I do not see what means we have at present for instituting a comparison between different localities of these three curved surfaces with the level of the general disc.

So far we have been dealing simply with the visual appearance of spots, and I think it is clear that even from this point of view we cannot speak in any rigid sense of their level. But there are other phenomena of which we must take count, which render the difficulty greater. I refer more especially to the results which the spectroheliographs of M. Deslandres and Prof. Hale have given us. These have shown us on the H and K lines vast masses of glowing gas, completely hiding great spots which yet to direct observation gave no hint of their presence. Other lines have shown us, or might show us, corresponding phenomena, for bright lines are a frequent item in spot-spectra. These glowing gases are above the spot and are intimately connected with its structure, and though they fail to reveal themselves to ordinary telescopic or photographic enquiry, yet in other methods of observation they produce their full effect. The experiments of Prof. Langley and of Mr. W. E. Wilson seem to show clearly that the radiations with which they are concerned emanate from a region as certainly above the general photospheric surface as are the summits of the faculæ, since, like them, they suffer less diminution in intensity at the limb; no doubt because these elevated strata of intensely heated gas, which our eyes ignore, produce their full effect on the radiometer.

Relatively to the entire Sun we can only regard a spot as a surface stain, a region intrinsically darker than its surroundings, and practically at the level of the general surface. Regarded by itself, a Sun-spot is a highly complex phenomenon, with its several parts each probably convex to the mean photosphere—the umbra overlapped by the penumbra, the penumbra by the photosphere, and yet the umbra sometimes attaining at its crest a greater elevation than either. With it and above it there often rise strata of glowing gases, integral parts of the spot, though escaping our vision.

Lastly, to complete our difficulty in assigning a definite level to spots, we have to reckon with the fact that in mid-Sun we are looking through a shallow vertical structure: near the limb through a very deep horizontal one; and the two are entirely different presentations. E. WALTER MAUNDER.

Periodical Comets due in 1897.

MR. LYNN's interesting notes on the periodical comets expected to return in 1897 may be supplemented by observing that Swift's comet will be invisible, and that D'Arrest's comet will be unfavourably placed, though it may possibly be seen.

In the case of comets having their perihelia outside the Earth's orbit (as Swift's 1880 IV., D'Arrest's 1851 II., and Brooks' 1886 IV.), they are visible to the best advantage when the Earth's longitude is nearly the same as theirs at about the time of their perihelia. The long. of per. of Swift's comet is 43°, which it reaches in May next; but as the Earth will be in the directly opposite region of her orbit on the other side of the Sun, the comet will be invisible, being obliterated in the solar rays.

D'Arrest's comet will arrive at perihelion (long. 319°) in May, as it did in 1877, and about 3 months before the Earth reaches the same longitude, so that the difference separating the two bodies will be very considerable and the comet if seen at all will be very faint. At its last return in 1890 it was accidentally re-discovered by Mr. Barnard on Oct. 6, 19 days after it had passed its perihelion.

This comet is observed to the best advantage when it comes to perihelion in August, and can also be very well seen when it returns in July (as in 1851) and in September (as in 1870 and 1890). At intervals of 20 years the comet visits us under similar circumstances, three of its revolutions being nearly equal to that period of time.

Brooks' comet 1886 IV. ought to return at about 1897 August, if Oppenheim's orbit (Ast. Nach. 3064) correctly represents the real path. The period he gave was 5·5954 years, and the comet should have returned in 1892 January, but the conditions were all against its being seen. This object does not appear to have been very well observed in 1886, several other brighter comets being visible at the same time, and there is much uncertainty as to the exact time of its revolution. If the comet is ever seen again, as it is very likely to be, it will probably be picked up in an accidental manner by some comet-seeker when not looking for anything special.

A glance at the table of periodic comets proves that a comparatively large number of these bodies may be expected during the next few years. In 1897 there are 4 due, and no less than 11 appear likely to visit us in the years 1898 and 1899 as follows:—

Date.	Comet.	Date.	Comets.
1898 April...	Pons-Winnecke.	1899 Jan. ...	Denning (1881 V.).
May ...	Encke.	Mar. ...	Tempel (1866 I.).
June ...	Swift (1889 VI.).	April...	Barnard (1892 V.).
June ...	Wolf.	May ...	Tuttle (1858 I.).
Sept....	Tempel (1867 II.).	May ...	Holmes (1892 III.).
		July ...	Tempel (1873 II.).

Bristol, 1897, Jan. 7. W. F. DENNING.

Thomas Gwyn Empey Elger.

It is with feelings of the deepest regret that we have to announce the death of Mr. T. G. E. Elger, the renowned selenographer, whose lucid notes descriptive of the lunar surface have for many years formed such a valuable contribution to this magazine and to astronomy generally.

Mr. Elger, born in 1838, was descended from a family established in Bedford for many years, but whose original home was in Norwich. His father was an architect in considerable practice, and several of the public buildings in Bedford were built from his designs. He was three times Mayor of the town, an office which his father, grandfather of our late contributor, had also filled many years earlier.

Thomas Gwyn Elger, after being educated up to the age of 13 at Bedford Grammar School, was placed under the care of a private tutor for some years, and afterwards entered at King's College, London, where he studied mathematics under Prof. de Morgan. After completing his course here Mr. Elger adopted the profession of engineer, and was taken into the office of Mr. John Fowler, under whose direction he had experience in many large engineering works. He took part in the construction of several railways in the north of England, the Menai Bridge, and the Underground Railway between King's Cross and Edgeware Road; and when he went into practice on his own responsibility Mr. Elger acted as engineer for a firm of contractors who were engaged in laying railroads in Denmark, until this work was stopped by the outbreak of war over the Schleswig-Holstein question in 1864.

This stoppage, and the fact that he had succeeded to his patrimony, seems to have induced Mr. Elger to give up engineering and to develope his taste for astronomy and science generally. About the year 1866 he settled with his mother at a house in Caldwell Street, Bedford, where he set up his first observatory; shortly after he removed to Manor Cottage, Kempston, where he also erected an observatory in the garden, containing a reflector by Calver of 8½ inches aperture, and also a 4-inch refractor by Cooke. He was elected a Fellow of the Royal Astronomical Society in 1871, February, and his first contributions to astronomical literature appear to have been on subjects other than selenographical, for in 1873 he contributed to the 'Monthly Notices' a note on the colours of the components of γ Delphini, and later, in the same year, we find some observations by him on the planet Venus: but his name will always be associated chiefly with lunar astronomy, to which he devoted himself and his telescope, and for which his early training as a draughtsman particularly qualified him; the sketches of lunar regions and formations which have recently appeared in these pages, and which are copies of exquisite drawings made by Mr. Elger, are evidence of his skill in this direction. Mr. Elger took a prominent part in the work of the Liverpool Astronomical Society, now defunct, and when the British Astronomical Association arose on its ashes he was unquestionably the man to be asked to take charge of the Lunar Section; it is in the publications of these two Societies and in this magazine where his lunar researches are chiefly to be found, excepting, of course, his own book 'The Moon,' published in 1895, an enduring monument of much laborious and painstaking work.

Mr. Elger interested himself in other pursuits besides astronomy. He was an ardent antiquarian, and possessed a fine collection of flint implements, old pottery, and other antiquities, which, it may be added, he proposed to present to his town, if the authorities would establish a public museum. He was also an active member of the Bedfordshire Natural History Society, of which he seems to have been the moving spirit during the ten years of its existence, after which it was absorbed by the Literary and Scientific Institute. Mr. Elger read many papers before this Society, not only on astronomical, but also on microscopical and antiquarian subjects.

It will be gathered from the above that Mr. Elger did his duty to his fellow townsmen by communicating to them the result of his scientific researches; no less conscientiously did he do his duty as a citizen. Like his father and grandfather he served as Mayor of the town, he was on the Commission of the Peace, and appears to have filled a place on many committees, where his experience and judgment were valuable.

Mr. Elger married in 1880 Miss Gissing, the daughter of a solicitor of Bedford who was afterwards Registrar of the town. He was taken with paralysis of the left side on December 29 last, which passed off, but left him in a state of weakness, from which he died on January 9, aged 59, leaving a widow and two young sons.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Ball's supposed Discovery of the Duplicity of Saturn's Ring.

GENTLEMEN,—

So much has been said on this subject in previous numbers of the 'Observatory,' that it would seem hardly possible that more should remain. But as it is desirable to be exact in matters of fact, I feel I must request you to insert these few additional lines. It will be remembered that it was in November 1880 ('Observatory,' vol. iii. p. 611) that I first called attention to the omission of the figure of Saturn and his ring sent by W. Ball to Sir Robert Moray, from the Note by the latter in No. 9 of the 'Philosophical Transactions.' I then remarked that the plate which contained that representation had been omitted from at least three copies of the 'Phil. Trans.'; and in the 'Observatory' for November 1882 (vol. v. p. 331) I stated that it was not to be found in eleven copies of the volume, nine of which I had consulted myself. Meantime, however, it had been found in several other copies, and the discussion which arose and proved incontestably that Ball was not (as had been supposed by some) the discoverer of the principal division in Saturn's ring is now matter of history. But happening recently to be turning over, for another purpose, a much later volume (xvii. for 1693) of the copy of the 'Phil. Trans.' which formerly belonged to Sir Joseph Banks and is now in the Reading-Room of the British Museum, I found to my amazement the missing plate with Ball's drawing of Saturn there. It is opposite the contents-page of No. 199 (April 1693), and has evidently been inserted after the volume was first bound, part of the plate (not that containing the drawing) being slightly injured. It is not unlikely, therefore, that something similar may have been done in other copies, for when making my search it did not occur to me (I can hardly be blamed) to look so far ahead as twenty-seven years. But this may account for the erasure of the fly-leaf note of the omission which I mentioned in the second letter above referred to ('Observatory,' vol. v. p. 332).

The University of Cambridge has recently published in a collected volume those of the late Prof. Adams's papers which appeared during his lifetime. One of these is that contributed to the 'Monthly Notices' of the R. A. S. for the month of January 1883, in which he describes the paper-cutting found at the Royal Society from which the drawing was made, and suggests that the folding of this caused those curious shallow notches or depressions at the extremities of the minor axis of the planet, which led Sir Robert Moray to fancy that it was surrounded by two rings rather than one. No editorial notes accompany these papers; but I offered reasons in the 'Observatory' for June 1883 (vol. vi. p. 185) for not accepting without reserve Prof. Adams's ingenious explanation. But at any rate it is quite certain that the duplicity suspected by Moray is of a very different kind from that which really exists.

Yours faithfully,

Blackheath, 1896, Dec. 31. W. T. LYNN.

The Geminid Meteor-shower 1896.

GENTLEMEN,—

Prof. A. S. Herschel's note ('Observatory,' Jan., p. 76) on the recent activity of this stream is fully corroborated by several other descriptions of the shower.

Mr. J. Evershed writes me that on Dec. 11, while returning from the R. A. S. meeting, and in the train between Croydon and Kenley, he counted 9 Geminids in about 10 minutes. On arriving home he began a watch at $12^h\ 50^m$, looking in the direction of the radiant, and saw 32 meteors in half an hour: about 10 of these were 1st or 2nd mags., and 5 faint ones belonged to other systems. The radiant appeared to be badly defined in the region of Pollux at about $117^\circ\ +25^\circ$.

Mr. H. Corder gives the following results of his observations at Bridgwater:—

Date.	Period of watch.	Total No. of Meteors.	Geminids.	Radiant.
Dec. 9	8h 30m to 12h 0m at intervals.	35	22	111° +34° 115 +32 123 +34
Dec. 11	10h 0m to 15h 0m	122	80	110 +34 112 +30
Dec. 12	14h 0m to 16h 15m	30	21	111 +33

Mr. D. Booth at Leeds also saw these meteors. On the night of Dec. 11 he counted 20 Geminids from a radiant 2° S.W. of Castor: at 12h 24m he recorded a brilliant meteor about 2 × ♀, shooting from 55° + 35° to 25° + 25°, and clearly belonging to the strong shower then in progress.

Yours faithfully,
W. F. Denning.

Bristol, 1897, Jan. 7.

OBSERVATORIES.

Melbourne.—Mr. Baracchi, the Director of the Melbourne Observatory, has lately sent us a copy of his first Report, which covers the period from 1895 May 31 to 1896 June 30. The new Director did not actually take charge of the Observatory until 1895 July 1, but the last month of Mr. Ellery's control, which was not covered by his last report, is included in this.

The immediate fact to be gathered from the Report is, as it has been for several years past, that the Observatory is suffering terribly from want of money. Possibly this is unavoidable, owing to the financial circumstances of the Australian colonies just now, but it is much to be hoped that when the time comes when things are more prosperous that the Observatory will be one of the first institutions to feel the benefit. There are several directions in which money might be spent with advantage. There are eleven years of observations with the transit-circle, made under Mr. Ellery's direction 1884–95, waiting to be made into a catalogue; these would, in the ordinary course, have been published as annual catalogues, but the necessity for retrenchment has prevented this. There are observations of terrestrial magnetism, extending over 30 years, which should be reduced and discussed. As this is the only institution in Australia where magnetic observations are carried on, these would form a valuable contribution to magnetic science, and, besides, this subject is especially to the taste of the present Director.

The Great Melbourne Reflector is still lying idle for the want of a man, or possibly two, to work it. This is, perhaps, an expensive item, and may be regarded as a luxury rather than a necessity. A subject leading to a more pressing demand for money is the Astrographic Chart. The energy of the Assistant in charge of this will be apparent from the fact that all the catalogue plates (1149 in number) except 81 have been taken, but the measurement and reduction of these is certainly impossible with the present staff. Provision has been made in the estimates for a new assistant, who will be partly engaged on astrographic work, but this is scarcely sufficient.

Turning to the record of work done, we find that with the transit-circle, besides 1045 observations for clock-error and adjustment, about 1270 stars have been observed in both elements, as fundamental stars for the astrographic catalogue. The practice at Melbourne is to select five stars on each plate as fundamental; the resolution passed at the recent conference at Paris, which demands ten such stars, therefore doubles Mr. Baracchi's estimate of work for this purpose. Speaking of the Transit-circle, there is a curious fact recorded. The variation of the level-error from 1890 to March 1895 showed a distinct period, the curve having a maximum ordinate in May or June, and a minimum in December or January. In March 1895 the instrument was reversed on its pivots, and during the following year this period quite disappeared, the level descending gradually and continuously from $+57''$ to $-66''$. With the Astrographic Equatoreal, 351 plates were exposed; deducting from these the plates not considered good and those taken for adjustments, there remain 214 catalogue plates and 40 for the Chart. The photoheliograph has been used with the reduced efficiency mentioned in Mr. Ellery's last report, photographs being only taken when conspicuous spots appear on the Sun. The great reflector has not been used during the period covered by the Report, but some improvements, optical and mechanical, have been made. The Director does not speak very hopefully of the prospect of using this just yet.

The meteorological service appears to make large demands on the limited staff. Mr. Baracchi thinks that the collection of results of rainfall is of importance, and has increased the number of stations which send to the Observatory. He is also making arrangements to cooperate in the work proposed by the Meteorological International Committee, of making observations of clouds at both ends of a base-line to determine their height and velocity.

HONGKONG.—The recently published volume of "Observations and Researches made at the Hongkong Observatory in the year 1895" contains the continuation of the elaborate investigation of the Typhoons of the China Seas by the Director (Dr. Doberck) in conjunction with Mr. Figg for the years 1890, 1891, 1893, and 1895. This investigation which has been in progress since the establishment of the Observatory in 1884 is now practically completed, and a revision of the laws of storms based upon it is now in progress, and will probably be finished in time to be utilized for weather forecasts in the present year (1897). 514 transits were observed by Mr. Plummer in the course of the year, and the time-ball was dropped with very few failures. The magnetic observations have been continued. There were slight shocks of earthquake on the 30th of August, and other shocks also occurred during the following night.

Only half the normal amount of rain fell in 1895 (45·83 inches as compared with an average of 90·17 inches), but this excessive drought had no influence on either pressure or temperature. The deficiency of rainfall was very marked in the months of May, June, August, and September. Sunshine was recorded for 2048 hours during the year, being 149 hours in excess of the average amount. The maximum temperature for the year was 94°·0 in September, and the minimum 43°·5 in January. The monthly tables of meteorological results are given in the usual complete and elaborate form.

MAURITIUS.—The report of the Director of the Royal Alfred Observatory, Mauritius, for the year 1894, is also to hand. This gives results of the observations as printed in detail in the "blue book," from which we learn the following particulars:—Bright sunshine was recorded on 2786 hours, being 63 per cent. of the possible duration. This is very slightly in excess of the average amount. The highest shade temperature in the Stevenson screen was 93°·5 and in the thermograph screen 92°·2. The lowest temperatures under the same conditions were 50°·6 and 51°·0 respectively and the mean annual temperatures 74°·3 and 73°·2. The rainfall amounted to 48·9 inches on 196 days, and this amount is almost the same as the corresponding average value. In January the rainfall was 7 inches above the average and in March 4 inches below. The greatest fall in one day was 3·70 inches on January 11, and the greatest intensity of rainfall occurred on the same day, amounting to 1·76 inch in 45 minutes.

Storm warnings were published on numerous occasions. Details of magnetical observations are given, and the results for 15 preceding years are also included.

Sun-spots were seen on 343 days and 568 photographs were obtained. The number of spots was rather larger in 1894 than in 1893.

This report is, as usual, very badly printed, and is full of glaring typographical errors.

PUBLICATIONS.

THE GEODESY OF SOUTH AFRICA.—In 1752 the Abbé de Lacaille having finished the observations for his star catalogue turned himself to terrestrial measurement. He determined by observations of meridian zenith distance of stars the latitude of his station at Cape Town and also of a station at Klipfontein approximately 1° 13′ further north; he compared this difference of latitude with its corresponding length deduced from a triangulation depending on a measured base-line. The length of a degree of latitude at 34° S. resulting from this comparison was for some years a source of perplexity to geodesists, as this was much greater than a degree in similar latitude in the northern hemisphere.

This apparent discrepancy was partly explained away by Sir Thomas Maclear, who, when he was in charge of the Cape Observatory, undertook to verify the result of, and at the same time extend, Lacaille's arc of the meridian. After much labour spent in identifying points of the original survey, when it was found impossible to exactly identify the ends of the base-line, Maclear measured a new base nearly on the site of the original one and connected this by triangulation with the extreme points of Lacaille's survey, and extended the triangulation so that it covered an astronomical amplitude in a north and south direction of 4° 37′. The result of this was to show that the length of a degree of latitude in Cape Colony agrees with that of a degree in the corresponding latitude in the northern hemisphere. The discordance shown by Lacaille's measures was partly accounted for by a large local disturbance in the direction of gravity at Klipfontein. Sir Thomas Maclear's survey was completed in 1848, and the results were published under Sir George Airy's superintendence in 1866.

The next step in the cartography of South Africa was taken in 1859, when a party of engineers under Capt. Bailey was entrusted with the work of making a map of the south eastern part of Cape Colony: by a disastrous shipwreck the original observations were lost, and only abstracts remained of the result of two or three years' work.

The above synopsis of the state of geodetical science relating to South Africa is extracted from the Introduction to Dr. Gill's Report on the Geodetic Survey of South Africa *, lately presented to both Houses of Parliament, and is complete up to the year 1879, when Dr. Gill was appointed to the Cape Observatory. He, following the traditions of his predecessors, felt that it was his duty to extend the accurate survey of this part of the globe, so as to include with Cape Colony the territories of the Transvaal, Natal, and the Orange Free State. With this in view he approached the authorities of the governments of Cape Colony and Natal, and by February 1883 these colonies had come to an arrangement to jointly bear the expense of a scientific survey. A detachment of Royal Engineers, consisting of 14 non-commissioned officers and men, under the command of Capt. Morris and Lieut. Laffan, reached Durban in 1883 June, and the work was begun. There was a little hitch later in the year, owing to the fact that the Natal

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* Report of the Geodetic Survey of South Africa executed by Lt.-Col. Morris, R.E., C.M.G., in the years 1883–1892, under the direction of David Gill, H.M. Astronomer at the Cape; together with a rediscussion of the Survey executed by Sir Thomas Maclear, F.R.S., in the years 1841–1848, by David Gill, LL.D. Cape Town, 1896.

authorities had apparently misunderstood to what they had committed themselves. As the cost appeared more than was anticipated, the Secretary of Natal was inclined to be satisfied with a mere map of the Colony fitted for official requirements, instead of making the survey a contribution to accurate geodesy. This difficulty was got over, but the necessity of husbanding financial resources was in many cases an impediment to the work.

The skeleton of the work may be thus described:—Base-lines were measured at Natal, Port Elizabeth, and Kimberley. The length of the base-line measured by Sir Thomas Maclear, known as the Zwartland base, was adopted without alteration. The measured line in Natal was taken as the base for a series of triangles along a line approximately parallel to the coast-line, to the Port Elizabeth base; from here the triangulation followed the coast-line to Cape Town and the Zwartland base. The series of triangles then runs north to Calvinia and then east to Hanover, where it joins a chain joining the Kimberley base to that at Port Elizabeth. There is a further short tie-chain of triangles joining the lines between Calvinia and Hanover to that between Port Elizabeth and Cape Town.

The measured angles of the triangulation and all details of their reduction are given in Col. Morris's carefully drawn up Report. Needless to say, these observed angles have all been adjusted for the geometrical conditions applying not only to triangles, but also to quadrilaterals and polygons; the length of base-lines as determined by direct measurement have been compared with their lengths determined by triangulation from other bases, some of which comparisons it is convenient and may be of interest to quote:—

Base.	Measured in feet.	Log.	Computed from.	Log.
Kimberley.	14760·304	4·1690955	Port Elizabeth	4·1690809
Kimberley.	14760·304	4·1690955	Zwartland	4·1690816
Zwartland.	42819·065	4·6316371	Port Elizabeth	4·6316232

It will be noticed that the greatest difference, measured—computed, which is in the case of the Zwartland base, corresponds to a distance of less than 18 inches. There is a line about 33 miles long, joining Bushmansberg to Elandsberg, whose length has been determined by two distinct systems of triangulation. The difference between these determinations is about 3½ feet.

Besides the triangulation, astronomical latitudes of seventeen stations were determined, and also the telegraphic difference of longitude between the Cape Observatory and eight stations of the survey, the Assistants of the Cape Observatory helping in this latter work. Dr. Gill is not satisfied with some of these longitude results, on the ground of possibility of variation of personal equation of the observers which has not been eliminated. However, these astronomical results, as well as some similar astronomical data found by Sir T. Maclear, have been compared with the geodetic, and the comparison forms an interesting chapter of the volume. Buffelsfontein, near Port Elizabeth, is the origin of the geodetic survey, and the longitude of this place from the Cape Observatory having been determined from the triangulation, the geodetic latitudes of twenty-six stations, whose latitudes had been determined astronomically, either by Col. Morris or by Sir T. Maclear, were computed, also the geodetic longitudes of the nine stations at which operations for longitude had been made. These geodetic results were computed with Clarke's elements of the Earth, and show differences astronomical − geodetic latitude ranging from $-7''{\cdot}90$, to $+10''{\cdot}74$, and similar quantities for longitude ranging from $-0''{\cdot}53$ to $+8''{\cdot}12$. The geodetic results were then computed with the elements of the Earth deduced by Airy, which showed the differences for latitude to have much the same range, but the nine values for astronomical − geodetical longitude, on this assumption, ranged between $-1''{\cdot}16$ and $+6''{\cdot}04$; or taking the two longitude arcs which are best determined, the astronomical amplitude of these is greater than the geodetic as computed by Clarke's elements, by about 4″, whilst with Airy's elements the excess is about 1″.

Discussing these results Dr. Gill dwells on the large part that abnormal local attraction plays in all geodetic work. He refers to apparent discordances which arise from local attraction at places where the surface contour would not lead any deviation to be expected, and suggests that the way to eliminate or diminish the effect of this is to multiply the number of astronomical stations, and also to measure areas of large extent. With this view he suggests that the present survey should be merely the beginning of a larger survey to extend eventually to the mouth of the Nile. We have reason to know that the energetic Director of the Cape Observatory is already empowered to make the next step in this great geodetic scheme. H. P. H.

RESEARCHES ON THE EVOLUTION OF STELLAR SYSTEMS, VOL. I. T. J. SEE.—As the greater part of the contents of this volume have been published from time to time in various periodicals during the past three or four years, they are already familiar to astronomers. It is, however, doubtful whether anyone has considered them as forming merely units in a definite scheme. That such they are is very apparent now the various papers are collected and formed into a complete volume. Dr See has always had "*Evolution*" in view and has determined—wisely or unwisely—to base his researches on orbits computed only by himself. These orbits form the body of the work and are, in their collected form, extremely valuable for reference both to observers and computers, beyond the present and future use for which they are destined by Dr. See.

Starting with the nebular hypothesis as enunciated by Laplace, the various modifications are reviewed down to the time when

"Darwin was led to consider not only the surface tides, but also those which arise in the body of a globe owing to its imperfect rigidity." The development of the theory of *bodily tides* and its application to the solar system by Darwin showed that "in the lunar-terrestrial system the action of tidal friction had been of paramount importance, but elsewhere the effects had been less considerable, owing chiefly to the small relative masses of the attendant bodies." This is the keynote of Dr. See's researches, which is expressed in his own words, thus—"The present volume is devoted mainly to *facts* as made known by the labours of double-star observers since the time of Sir William Herschel; the more theoretical inquiry into the secular effects of Tidal Friction and the processes of Cosmogony is reserved for subsequent treatment."

These facts are the 40 orbits referred to above and which have been computed by Dr. See. These, with the observations on which they are founded, are given separately and in full, while the resulting elements are also given at the end in a concise tabular form with various additional information, such as colour, magnitudes, light ratio, proper motion, orbital motion in line of sight, and inclination of the orbital planes to the plane of the Milky Way. With respect to this latter, we notice that Dr. See arrives at the same result as Miss Everett*, viz., that the orbital planes lie at all possible angles to the plane of the Milky Way, with no marked relation to the general scheme which distinguishes the arrangement of the stars and nebulæ. Now concerning these *facts*: we have always thought Dr. See just a little too ready to overweight the accuracy of his deduced elements, and a little caution is necessary when digesting the tabular matter on pp. 244–5. The $\pm x$ attached to the "Period" is a wee bit too sanguine, and requires considering from the point of view of Burnham's recent articles † on binary star orbits. However, this is only by the way and will not affect, to any serious extent, the use to which Dr. See will put them, and without a little elasticity combined with a great amount of enthusiasm the *main object* in view would be unattainable with the material at command.

Besides these orbits there are several theoretical papers dealing with the laws of attraction, parallaxes, spectroscopic observations, &c. There is also a good exposition of Kowalsky's method of determining a double-star orbit.

In Chapter III. we notice with pleasure that the spectroscopic study of binaries is brought prominently forward. We have always been of opinion with Dr. See that the "prosecution of these researches with the powerful spectroscopic appliances of the great telescopes of our time is an urgent *desideratum* of astronomy."

Finally, the following deductions, no doubt intended to form the corner-stone of Volume II., are given:—

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* Monthly Notices R.A.S., 1896 June—a discussion of 55 orbits.

† Popular Astronomy, 1896 Dec., 1897 Jan.

(1) Double-star orbits are highly eccentric, and are on the average twelve times that found in our planetary system.
(2) The stars of a system are comparable, and frequently almost equal, in mass.

The volume is dedicated to Benjamin Apthorp Gould, and, considering it therefore as an obituary tribute, the book comes opportunely. Also there is internal evidence that the observations have not been allowed to grow cold, but that the publication of the book has been carried on with praiseworthy rapidity. We offer our best congratulations to Dr. See on his handsome volume.

T. L.

COURS D'ASTRONOMIE *.—There is a story told of a well-known popular lecturer on astronomy who gave a course of a dozen lectures at a country town, and on being asked to give some more at the same place the next year, he replied that no change of sufficient importance had occurred in the sky to render further lectures necessary or desirable. By similar reasoning we may confidently predict that we shall never see another treatise from the pen of M. Baillaud, for in a volume of about five hundred pages he has skimmed over a range of subjects that would require several books for their adequate treatment. There is little, if any, internal evidence to show that the author is a practical astronomer: there is much to show that he is a lecturer; in fact the book is stated to be written for the use of students, and has evidently been compiled by amalgamating three sets of lecture-notes, on Spherical Astronomy, Gauss' Theoria Motus Planetarum, and on Disturbed Elliptic Motion.

To illustrate our opinion that the book is more adapted for the lecture-room than for the observatory, we might mention that there are no tables and very few numerical examples. His chapter on refraction would leave a reader without any idea of how to apply the tables in ordinary use. There is no explanation given of the practical methods of finding the parallax of planets, the constant of nutation, or the position of the ecliptic, which might very well have been given in any book on Spherical Astronomy. Gauss' work is not illustrated by numerical examples, so that this section of the book falls far below the treatise by Watson in usefulness. But it is perhaps in the third section, on the problem of three or more bodies, that the superficial character of the book is most conspicuous. The subject is not one that can be adequately treated in a hundred pages, and it was a pity to try to do so. Laplace's method, where the true longitude is taken as the independent variable, is quite out of date, and de Pontécoulant's theory had far better have been selected if the author felt obliged to confine himself to one. Why, too, is there a chapter on the satellites of Jupiter, if the author is not prepared to discuss the permanence of the curious relation between three of the mean

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* B. Baillaud, Director of the Observatory at Toulouse. Paris, 1896.

longitudes? M. Baillaud had accumulated materials which might have formed the groundwork of three good volumes; by tossing them into one bad one he has wasted them.

RESULTS OF RAIN, RIVER, AND EVAPORATION OBSERVATIONS MADE IN NEW SOUTH WALES DURING 1894. H. C. RUSSELL, B.A., C.M.G., F.R.S.—In 1870 Mr. Russell was appointed Astronomer at Sydney, and immediately (there being then only 5 stations reporting rain) began the expansion of the rainfall records. In 1893 there were 1287 observers; in 1894, 1329. Apart from the volume now before us, which contains 240 pages of very close printed matter, exclusive of temperature and rain charts, these statistics show how very hard Mr. Russell has laboured to obtain material for the discussion of a problem so vital to the colony.

Though much of the matter is tabular in form, and therefore appeals only to an earnest student of the subject, much interesting information is given in the introduction on the subject of local rainfall and drought, and of evaporation from wide expanse of lake, or sandy soil, grass prairie and gauge. We are warned not to be misled by the idea that the quantity of rain called the "average" is what may fairly be expected to fall in any given year. That is an error into which many in this country have indeed fallen lately, who thought that the abnormally heavy autumn rains, following on the drought of spring and early summer, was accounted for by the law of averages, and that the average rainfall in England of 27 inches was the law which must be obeyed.

Winds play an important part in the variation of rainfall in contiguous districts. Why in an extensive flat area, one small portion should not receive the same amount of rain as the parts near it, is not at first sight obvious; why it should do so two years running is still more difficult to fathom. Possibly, as Dr. Russell suggests, it may be traced to the presence of certain winds, if the question can be studied in a series of years. Similarly with regard to the amount of evaporation from Lake George, which is much greater than from the evaporator-gauge. The lake is lashed into breaking waves by strong winds and the spray is carried—a misty rain-like deposit—into the forest. The gauge, on the other hand, is protected by its small size from the wind.

The evaporation at Lake George has been found to bear an inverse proportion to the amount of rainfall in the catchment. This is what might be expected from two considerations, since not only is the evaporation less, the greater the humidity of the air, but also in a dry season it may be assumed that drainage from the surrounding district into Lake George may be neglected, and in wet one the water drained into it will materially affect the height of the lake.

A more important factor still in the average rainfall of a district is the altitude of the station, and especially if the ground rise rapidly to it. A general rule seems to be that the fall per mile gets less as the hills are left and is fairly regular.

The most distinguishing features of the volume are the charts of temperature and rainfall. These are four in number. The first is a map of New South Wales showing in black figures the average temperature for each month in the year; in each square degree a reticule of 12 squares covers each degree corresponding to the months of the year. The second map shows the mean shade temperature for the 4 seasons, the mean shade temperature for the year, and the highest and lowest shade temperature ever recorded in each square degree. The third has a larger or smaller circle described round each rainfall observatory, showing the number of inches in 18)4. A diagram in the north-west corner gives the rainfall of the colony for each year from 1871–1894. The fourth is a diagram showing the monthly distribution of rain in each square degree of New South Wales by means of 12 vertical lines representing the twelve months, the vertical length of each indicating the quantity of rain.

ON THE PREDICTION OF SOLAR ECLIPSES. By SHIN HIRAYAMA. (Reprinted from the 'Journal of the College of Science, Imperial University, Tokyo, Japan,' vol. ix. pt. ii. 1896.)—This work developes a method of investigating eclipses which closely resembles that of Bessel, but differs from it in the choice of fundamental axes. In this system the axis of z is the line joining the centres of the Sun and Earth: the plane of reference is drawn through the centre of the Moon perpendicular to the axis of z. The axes of x, y are in this plane, that of y being the intersection of the plane of reference with the Sun's declination plane, and that of x at right angles to this.

In Bessel's system the axis of z is the line joining the centres of the Sun and Moon, and the plane of reference is drawn through the centre of the Earth perpendicular to it. The two systems are therefore very similar, merely interchanging the Earth and Moon.

Mr. Hirayama developes very fully the method of deducing all the circumstances of eclipses on the new system of coordinates, which he says was first suggested by Gauss, and afterwards elaborated by Sawitsch.

Three tables are given at the end, from which the small terms arising from the spheroidal form of the Earth may be readily taken out.

NOTES.

COMET NOTES.—Perrine's Comet of Dec. 8 proves to be a comet of short period, the following elliptical elements having been deduced by F. Ristenpart from observations on Dec. 10, 22, 27, Jan. 2 (Ast. Nach. 3396). With the exception of ω, the elements closely resemble those of Biela's Comet, which are given for comparison:—

T = 1896, Nov. 24^{d}·743 Berlin M.T.

	Perrine.		Biela.
ω	163° 57′ 31″	1897·0.	223° 17′
☊	246 24 7		245 51
i	13 50 41		12 33
log *q*	0·04641	....	9·9348
log *e*	9·84340	....	9·8784
a	3·676	....	3·526
Period	7^{y}·047	....	6^{y}·62

Similar elements have been deduced by Messrs. Hussey and Perrine (Ast. Nach. 3399).

Ephemeris for Berlin midnight.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Jan. 31....	5	8	4	0	19	Feb. 16	5	47	34	1	57
Feb. 4....	5	18	37	0	43	20	5	56	32	2	21
8....	5	28	39	1	7	24	6	5	18	2	44
12....	5	38	19	1	32	28	6	13	57	3	6

The brightness during February diminishes from 0·11 to 0·04 of that at discovery.

Dr. Knopf has computed the following elements and ephemeris for Berlin midnight of Perrine's Comet of Nov. 2 (Edinburgh Circular, No. 52):—

T = 1897 Feb. 8^{d}·1763 Berlin M.T.

ω	172° 21′ 0″	1897·0
☊	86 17 51	
i	146 8 25	
q	1·06225	

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	′
Apr. 5....	17	59	32	72	10	May 19....	9	21	41	29	8
13....	12	42	42	77	38	23....	9	22	11	26	51
21....	10	8	10	63	7	27....	9	23	7	24	55
29....	9	35	9	49	8	31....	9	24	23	23	17
May 3....	9	28	30	43	36	June 4....	9	25	56	21	54
7....	9	24	42	38	57	8....	9	27	40	20	44
11....	9	22	40	35	4	12....	9	29	34	19	44
15....	9	21	47	31	50	16....	9	31	35	18	54

Brightness 6·1 on April 5, 0·3 on June 16.

Dr. John Riem suggests that a comet observed by the Chinese in B.C. 612 is very probably identical with Comet 1881 III. (Tebbutt), for which he recently found a period of about 2400 years. A. C. D. C.

MINOR PLANET NOTES.—Three new planets, DD, DE, DF, were discovered by M. Charlois on Dec. 31.

Herr Villiger made a search in the vicinity of (151) Abundantia in October last, to see if he could detect the companion planet whose existence Herr Groeben recently suggested, but without success (Ast. Nach. 3386). A. C. D. C.

UNIVERSAL TIME.—Last month we mentioned a possible development of the Universal or Zone-time system as to which we were not then able to write definitely for want of information.

We have since received the 'Bulletin de la Société Astronomique de France' for January, in which there is an account of a discussion at the Society on the adoption by France of the Greenwich meridian for the origin of longitudes and time-system.

It appears that M. Deville introduced in the Chamber of Deputies a proposal that the time of France should be that of the Greenwich meridian. The proposal was referred to a committee, who reported in favour of the adoption. Of the reception of this report by the Chamber itself we have not heard.

At the meeting of the French Astronomical Society on December 2 this subject gave rise to a long discussion. M. Boquet de la Grye, after remarking that he was surprised that scientific men of France had not been consulted by the Chamber, spoke strongly against M. Deville's proposal. He argued that the chief reason for the adoption of the English time was the maritime supremacy of England; but, said he, the time may come when some other country may be in this position, and how then? He suggested that the meridian of Behring Straits, of which the anti-meridian would cut Europe nearly centrally, should be adopted as initial.

M. Callandreau spoke in favour of France adopting the Greenwich meridian, on the ground that she would otherwise be left soon the only country which had not a time system based on Greenwich. Colonel Bassot pointed out that Spain, France, and Greece had not yet adopted the meridian of Greenwich. We believe that if France adopted the proposal, that Spain and Portugal would follow, but we feel that Col. Bassot touched a weak spot when he said that Ireland still continued to use Dublin time. It is time that this should be altered.

THE SYSTEM OF CASTOR.—Prof. Burnham has found time to discuss the measures of the binary system Castor, and has done good work in again pointing out some of the pitfalls. This pair has been under some kind of observation since Bradley's time, but has only been accurately measured since 1826, and now in 1896

Prof. Burnham cannot get an orbit, or rather he can get too many. Yet the following have been given seriously at different times:—

Date.	Computer.	Period.
1831	J. Herschel	252·66 years.
1841	Mädler	232·12 „
1845 ...	Hind	632·27 „
1846	Jacob	653·1 „
1847	Mädler	519·77 „
1859	Thiele..........	996·85 „
1875	Wilson	982·9 „
1877	Doberck	1001·21 „
1878	Thiele..........	720 „
1889	Mann..........	265·7 „

We are never tired of pointing out that premature orbits are misleading and do harm. Out of the numerous orbits that can be drawn satisfying the observations, Prof. Burnham draws two, one giving 457 years as period, the other 657. Of course he makes no attempt to give elements of an orbit.

THE Discussion of the orbit of τ Ophiuchi by Prof. Burnham in 'Popular Astronomy,' January 1897, shows that he has fairly set out on the work of landing us firmly on bed-rock with respect to our knowledge of these stellar systems. Treating the material in a similar manner as described above in the case of Castor, he draws three ellipses, each representing the observations equally well, but differing widely in character and period. One gives 274, another 265, and the third 467 years. In fact all we know is that the period is not less than 200 years, and may be much greater than any so far suggested. Former computers give:—

1846	Maedler	87 years.
1848	Hind	104 „
1870	Doberck	185 „
1871	Doberck	218 „
1895	See	230 „

THE series of measures of double stars made at Lick in 1896 is at first glance rather discouraging. These look better when separated into measures made with the 36-inch and those made with the 12-inch. These latter are very wide pairs, and are evidently made for some special object, although unfortunately this is not stated.

Wide pairs ranging from 6″ to 86″, with a mean of 12″, are better left to possessors of 5- and 6-inch telescopes. The 15 stars measured with the 36-inch are of quite another order, and show that Mr. Aitken is a keen observer; but why are there not more?

THE MARKINGS ON VENUS.—The following letter from Mr. Percival Lowell was addressed to one of the Editors personally, but there seems no reason why it should not be published, especially as it corrects a slight error in a previous number:—

"In the last 'Observatory'* I notice that the markings on Venus I sent you are summarized as being sometimes visible for days, and then sometimes not. This, however, is a mistake. They were permanently visible—very markedly so. What I said about atmosphere was such as applies to all observations. There was practically not a sigle day when Venus was observed when drawings of the markings could not be made. For scepticism, of course, I care nothing; people are naturally prone to disbelieve what they have not seen, and the distinctness of the markings in good air raises them above criticism. It is merely question of time when they and their consequences shall be generally recognized."

"Lowell Observatory, 1897, Jan. 12."

THE SOLAR ECLIPSE OF 1896, AUG. 9.—In 'Nature' for January 28 there is an excellent picture by Lt.-Gen. Nicolas Kaulbars of the corona seen last August by the expedition sent by the Russian Astronomical Society to Lapland. The picture gives the combined results of photographs, ten in all, taken with a refractor of 7 inches aperture, a camera of 4 inches aperture, and one of 2 inches aperture, together with the appearance as seen by the naked eye in a perfectly transparent sky. The corona is of the type associated with the minimum of sun-spots, the polar plumes being well shown both north and south. There is a long streamer in much the same place as in the pictures obtained by the Nova Zemlya expedition.

OBSERVING astronomers often feel the want of some means of knowing readily the approximate sidereal time when away from their observatories. The easiest way seems to be to carry a watch showing sidereal time as well as the usual time-keeper, and remembering this we call attention to an announcement in our advertisement pages by a firm who supply a cheap sidereal time-keeper with a 24-hour dial which is very much up-to-date. We have not tried one of these watches and therefore cannot speak as to their time-keeping quality, but call attention to their usefulness for the above purpose.

THE Directorship of the Observatory at Kiel, Germany, left vacant by the death of Prof. Krueger, has been filled by the appointment of Dr. Paul Harzer, hitherto Director of the Gotha Observatory. It may be remembered that Prof. Krueger's promotion to Kiel was also from Gotha. Dr. Harzer will also be Professor of Astronomy in the University.

THE death is announced of Prof. G. D. Weyer, Professor of Mathematics and Astronomy in the Kiel University. Prof. Weyer's work had been chiefly in the branch of Nautical Astronomy, and he had in early life been an assistant for a few years at the Hamburg Observatory. He was 79 years of age at his death, which took place near the end of last year.

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* 1896, December, p. 420.

Prof. Brown's observation of Sirius made on March 13th bears out the position of the companion as found by the Lick observers. At the time, Prof. Brown does not seem to have had very great confidence in his measures, and was unable to verify this place.

1896 March 13 .... Pos.-angle 190°·1 Dist. 4″·29

We learn from 'Popular Astronomy' that Dr. Hermann Davis, of Columbia University, who was recently in England with his talented wife, has undertaken, with the co-operation of the latter, to re-reduce all Piazzi's star-observations. This is a big work, and calls forth our heartiest wishes for success.

Dr. Wilhelm Valentiner, who has charge of the Karlsruhe Observatory, has been appointed Professor of Astronomy in the University of Heidelberg. In consequence of this the Karlsruhe Observatory is to be transferred to Heidelberg, where a building is being erected for this purpose.

Lord Kelvin and Prof. S. Newcomb have been elected honorary members of the St. Petersburg Academy of Sciences, and Lord Rayleigh has been made a corresponding member.

Prof. Asaph Hall and Prof. Lewis Boss have consented to be Associate Editors with Dr. Chandler of the 'Astronomical Journal.'

At the moment of going to press we learn that Professor Barnard will be present in person to receive the Gold Medal of the Royal Astronomical Society on February 12.

From an Oxford Note-Book.

On Friday, February 12, after the Annual Meeting of the Royal Astronomical Society, there will be a dinner at the Criterion Restaurant in Piccadilly, open to all Fellows of the Society and their friends (price *7s. 6d.* each, not including wines). The arrangements will, in fact, be the same as those announced in these Notes last year. Intending diners are requested to send their names either to the Secretary of the Society, or to Dr. Common, 63 Eaton Rise, Ealing, some few days beforehand, if possible, though it is hoped that those who cannot come to an early decision will not be deterred by this fact from coming to the dinner.

The last Annual Dinner of the Royal Society, on November 30, was not noticed in its proper place. It seemed to me rather a dull one. I find but three utterances jotted down in the note-book. The first was by the Speaker (Mr. Gully), who said with some solemnity that "the House of Commons has never refused to grant any money to Science recommended by the Government o the day." This does not, however, seem such a weighty remark now as it did in an after-dinner speech. Lord Rayleigh told a good story about some lynchers in the Wild West who, by mistake, lynched the wrong man, and, feeling that some apology was due to the widow, submitted it in this form: "We must own, ma'am, that you have the advantage of us this time." The story is good, but I have never been able quite to see the application of it to Lord Rayleigh's resignation of his Secretaryship, which was the point under discussion. Perhaps the Lord Mayor made the happiest little hit of the evening when he addressed the Society as "This Society, which knows everything and discovers the rest."

THE year 1896 has afflicted Astronomy heavily: the loss of such men as Gould, Gylden, Krueger, Axel Möller, H. A. Newton, and Tisserand will be severely felt. Especially has one department of Astronomy suffered which could ill afford it,—the department of Celestial Mechanics. Two names of the six given above, those of Gylden and Tisserand, belong to the first rank of those carrying on the work at the classical problems of planetary motion. Their work was very different in character, Tisserand having devoted his best years to a gathering together of what was already known and done (for though his 'Traité de Mécanique Céleste' is by no means a compilation and contains much original work, it is inspired throughout by known methods); while Gylden struck out an entirely new line for himself, so new and strange that many still shake their heads at his enterprise. Fortunately for the world both men lived to achieve completion of their work. Volume iv. of Tisserand's 'Mécanique Céleste' was published early in 1896; and though much of Gylden's work is still unpublished, we learn from his friends that it is practically complete in MS., and almost ready for press.

IT was remarked of Sir John Herschel that his imagination could "clothe the description of so bare a thing as a Catalogue of Stars with the amenities of human interest and the graces of a poetic eloquence." In the introduction to his "Third Catalogue of Variable Stars" (Ast. J. No. 379), Mr. Chandler does not attain or even attempt any flights of poetry or eloquence; but he makes one remark of considerable human interest, on the subject of organization of astronomical labour. After calling attention to the uniformity and completeness of the observations of Variables during the last few years, very few stars in the northern hemisphere having been seriously neglected, he notes the fact that this harmonious development is due to individual and volunteer effort, and not to any concerted scheme of cooperation. Indeed he is of opinion that this satisfactory result "could hardly have been reached so effectively by a formal organization of work, directed from headquarters, prescribing and circumscribing the operations of each participant, and destroying, by its benumbing influence, the enthusiasm which springs from the individual initiative of the observers themselves." Many of us will not be prepared to go this length; for instance the officers of the British Astronomical Association would probably take exception to these remarks. But founded on actual facts as they are, they seem worthy of digestion.

THERE is a small matter concerning the identification of stars to which it may be well to call attention. A star is often quoted by its B.D. number (say B.D. + 27° 2305) without adding anything further. Now the 'Durchmusterung' is an expensive work and is, moreover, becoming scarce, and it is seldom in the possession of an amateur, who is thus put to the inconvenience of visiting a library when he wishes for the position of such stars. Would it not save much trouble in the end if the R. A. and Dec. for 1855·0 (or better for 1900·0 perhaps) were added in all such cases? The same remark applies in a modified degree to other Catalogues, but the case of the 'Durchmusterung' is an important one.

A MISPRINT sometimes makes a considerable difference, especially when it takes the form of an extra cypher. The 'Daily Telegraph' of January 22, in reviewing very favourably the 'Autobiography of Sir George Airy,' remarks:—"After all his income never exceeded £12,000 (*sic*) a year, and he retired on an annual special pension of £1100." The reviewer must have been thinking of a lawyer or a doctor.

THE possessor of a private observatory at some little distance from his dwelling told me the following story the other day. A friend called at his house while he was busily engaged in sweeping for Comets at the Observatory; and the servant offered to conduct the caller thither, knowing he would be welcome. She had apparently, however, over-estimated her own knowledge of the locality, and was in some doubt how to find the spot; but ultimately arrived there and triumphantly announced the fact by saying:—"Ah! this must be it, sir! because it's opposite the Moon!"

Another instance of the advisability of announcing a conclusion, but not giving your reasons.

ON January 9, the 'Academy' sent out to a number of prominent men and women a circular and a postcard, asking the recipient to write on the postcard the titles of the two books which had most pleased and interested him in 1896. Over 50 replies were received, of which the following may amuse our readers:—

Prof. NORMAN LOCKYER.
Mars, by Percival Lowell; *Sentimental Tommy*, by J. M. Barrie. (No time for reading seriously.)

Sir ROBERT BALL, Astronomer Royal (*sic* !!).
Lecky's *Democracy and Liberty*.
Brehm's *From North Pole to Equator*.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 251. MARCH. 1897.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, February 12, 1897.

Dr. A. A. Common, F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., and E. W. Maunder.

Mr. Maunder read the Minutes of the last Anniversary Meeting and of the Special General Meeting on 1897 January 8, which were confirmed.

Mr. C. Thwaites read the report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1896 and certified the same to be correct. The cash in hand on 1896 Dec. 31 was £201 17*s*. 6*d*., as against £349 1*s*. 8*d*. on 1896 Jan. 1. They had inspected the books and other property of the Society, which appeared to them to be in good condition. The Auditors added to their report a note to the effect that they considered that the amount (£5500) for which the property is insured does not cover its value, and they thought that this amount should be increased.

The President. Before submitting this report of the Auditors for the approval of the Meeting, I should like to state my opinion that for the Auditors to make this recommendation is entirely beyond their province, but I have not the slightest doubt that it is one worthy of consideration, and as such will no doubt be dealt with by the Council.

The Secretaries read extracts from the seventy-seventh Annual Report of the Council. The loss to the Society by the death of Fellows and Associates during the year has been great. The obituary list contains the names of the following Fellows:—G. P. Bidder, Rev. Henry Collins, G. R. Farncombe, A. H. Howe, George Hunt, J. H. Jones, J. R. Jones, Thomas Keig, S. H.

Parkes, Sir John Pender, W. L. Sutton, Sidney Waters; and of Associates, B. A. Gould, Hugo Gylden, Adalbert Krueger, Axel Möller, H. A. Newton, and F. F. Tisserand.

The President announced that the Council had awarded the Society's Gold Medal to Prof. E. E. Barnard for his discovery of the fifth satellite of Jupiter, and for his other astronomical work. He stated that it had been expected that Prof. Barnard would have been present to receive the Medal, but unfortunately, owing to a fog in the Channel, he was at that moment in a vessel lying off the Isle of Wight, which was unable to reach Southampton.

After delivering his address giving the grounds for this award, the President handed the Medal to the Foreign Secretary, Dr. Huggins, to hand to Prof. Barnard.

The President. I have another pleasant duty to perform; this to mention an award now made for the first time. This arises from a bequest by the late Mrs. Jackson-Gwilt, and consists, as the Fellows are no doubt aware, of a bronze medal and a gift of money.

Your Council has awarded this gift to Mr. Lewis Swift, and I am sure that I need not say anything to you of Mr. Swift's astronomical labours nor of the peculiar circumstances that will, I hope, make the gift doubly acceptable to him. In fact, as I have the opportunity of setting the precedent in this matter, I propose to refrain from giving an address: for, though the Society has been accustomed to hear an address read in reference to the award of the Gold Medal, I think that there are reasons why this custom should not be followed in the case of this gift, and so I will only ask the Foreign Secretary to forward this Medal and the gift to Mr. Swift, with the best hope of the Society that better times may come to him.

Lord McLaren. It is my duty to propose the usual resolution: "That the Report now read be received and adopted, and that it be printed and circulated in the usual manner, together with the President's Address and the Report of the Auditors." It is quite unnecessary for me to say anything in support of this resolution. The Report has evidently been prepared with all the care and accuracy that has always characterized the reports of the Council, and which have made the reports such admirable history; and we must all have been interested in the Address which the President has just read to us.

Admiral Sir Erasmns Ommanney. I have great pleasure in seconding this motion. I think that we shall not easily forget the able and interesting address which we have just had from Dr. Common.

The President appointed Mr. V. J. Bouton, Mr. A. Cottam, and Major Kingsley Foster as Scrutineers of the Ballot.

The Ballot was then proceeded with, and the following gentlemen were elected to serve as Officers of the Society for the ensuing year:—

President: Sir R. S. Ball. *Vice-Presidents*: Dr. A. A. Common, Dr. G. H. Darwin, E. W. Maunder, Dr. Isaac Roberts. *Treasurer*: E. B. Knobel. *Secretaries*: H. F. Newall, Prof. H. H. Turner. *Foreign Secretary*: Dr. William Huggins. *Council*: Capt. W. de W. Abney, W. H. M. Christie (Astronomer Royal), Dr. A. M. W. Downing, F. W. Dyson, Dr. J. W. L. Glaisher, Capt. E. H. Hills, W. H. Maw, Capt. William Noble, G. M. Seabroke, E. J. Stone, Dr. E. J. Spitta, Dr. G. Johnstone Stoney.

The result of the Ballot was read by Mr. Cottam, and Sir Robert Ball then took the Chair.

Rev. F. Howlett proposed, and *Major Kingsley Foster* seconded, that "the thanks of the Meeting be given to the President, Vice-Presidents, Secretary, and other Members of the Council who now retire, for their services in promoting the objects of the Society."

The motion was carried by acclamation.

On the motion of *Dr. R. Bryant*, seconded by *W. T. Lynn*, a vote of thanks was accorded to the Auditors of the Treasurer's accounts, and the Scrutineers of the Ballot.

The following gentlemen were elected Fellows of the Society:—

George Banks, 4 John Street, Bedford Row, W.C.; *John Chas. Clancey*, F.R.G.S., F.S.I., Assistant Director of Land Records and Agriculture, Burmah, 7 Grafton Square, Clapham Common, S.W.; *William Fraser Doak*, M.A., Nautical Almanac Office, 3 Verulam Buildings, Gray's Inn, W.C.; *Rev. Edward Spry Leverton*, M.A., School House, Kirkham, Lancashire; *Ernest Frederick John Love*, M.A., Lecturer in Physics in the University of Melbourne, Australia; *William Arthur Smith*, 78 Hagley Road, Edgbaston, and 94 Charlotte Street, Birmingham; *Edward Weldon*, 10 Morden Road, Blackheath, S.E.

The following Candidates were proposed for election as Fellows of the Society:—

John Franklin Adams, Wimbledon (proposed by L. Becker).

Rev. Sherrard Beaumont Burnaby, M.A., The Vicarage, Hampstead, N.W. (proposed by B. Woodd Smith).

John Sisson Slater, M.A., LL.D., Barrister-at-Law, 1 Garden Court, Temple, E.C., and Seafield House, Lytham (proposed by Sir R. S. Ball).

Rev. John Spence, Minister of Chelsea Chapel, 27 Walpole Street, Chelsea, S.W. (proposed by W. Willett).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The third Ordinary Meeting of the current Session of the British Astronomical Association was held at University College, Gower Street, on Wednesday, January 27th, *Mr. N. E. Green, F.R.A.S., President*, in the Chair.

The President referred to the death of Mr. T. G. Elger, who had from the formation of the Association been a member of the Council and Director of the Lunar Section. A letter of condolence to Mrs. Elger was read and approved.

Mr. J. Evershed read a paper on "the Cause of the Darkness of Sun-spots." He said that the blackness of spots had been generally attributed to dense masses of cool vapour which absorbed the light of the underlying photosphere. He had always felt that this was a very unsatisfactory explanation and he was glad to see from the discussion that had taken place at the previous meeting that this somewhat crude idea had been practically abandoned and that Mr. Maunder had substituted a view which seemed much more in harmony with the observed telescopic appearance of spots. It appeared to the writer a reasonable hypothesis that the interior of the Sun was so inconceivably hot that its radiation took place in wave frequencies of a higher order altogether than those which could be perceived by eye or photography, the visible radiations being relatively feeble. This assumed that in a radiating body at high temperature the longest waves tended to diminish in intensity with increase of temperature. It would seem not impossible, therefore, that in the radiation of a sun-spot this relatively feeble region of the spectrum had crept up into the shorter visible wave-lengths following in the wake of the rays of greatest energy, which had travelled far up into the ultra-violet. It seemed very desirable that solar-spectroscopists should follow the spot-band far into the invisible regions by photographic and bolometric methods, so as to discover the true significance of Wilson's results.

Capt. Noble remarked that when astronomers talked about the blackness of sun-spots they were apt to forget how perfectly relative that darkness was. He reminded the meeting that the brightest artificial light we were able to produce looked like a black spot on the Sun. Fizeau's and Foucault's experiments showed that the photosphere was 146 times brighter than the lime-light.

Mr. Maunder thought that Mr. Evershed's was a most thoughtful and suggestive paper. The difficulty with which Mr. Evershed had dealt was a very real one, for it was perfectly clear, as the writer of the paper had pointed out, that if they had in a spot a breach in the photosphere, and they had the glowing gases revealed to them that were below the photosphere, they would expect that the failure in the radiating power of these gases would be compensated by their greater transparency. The method in which Mr. Evershed had suggested they might get over that difficulty was one that was quite novel to him (the speaker) and was certainly worthy of the most careful investigation.

Mr. Evershed had also drawn attention to a very important point which had too often escaped the notice of spectroscopists, namely, that the spot-spectrum as they knew it in the spectroscope was certainly affected by the illumination of our own atmosphere. Of that effect they ought to take full account. He trusted the suggestion with which Mr. Evershed concluded his paper, that the spot-spectra should in future be followed up in the extreme ultra-violet and infra-red, would bear fruit.

The Secretary read a paper on "The Positions of Sun-spots" by the *Rev. A. L. Cortie.* The writer remarked that a simple method of determining the heliographic latitude and longitude of sun-spots was by the use of orthographic projections of the parallels and meridians on discs of the same size as the drawings of the image of the Sun to be measured, prepared for the positions 0° to ± 7° of angular tilt of the axis of the Sun relatively to the visible limb.

Mr. Maunder thought that for many purposes the projection method was quite sufficient where no great accuracy was required or in investigations where a large number of spots were being treated together, so that small errors would go out in the mean. Mr. Maunder then showed on the screen some photographs of the great group of sun-spots of September last taken at Greenwich and in India, and also some views of the great group of January, the latter of which covered an area of 2,800,000,000 square miles.

Mr. H. F. Griffiths read a paper on "The Oudemann Sea of Mars," in which he drew attention to the strong evidence we appeared to have of extensive changes that had taken place on Mars since the planet was first observed, and drawings of sufficient detail for comparison made. These changes were most conspicuous in the so-called "seas." By the aid of lantern-slides thrown upon the screen, drawings of the Oudemann Sea covering a period of 17 years were contrasted.

Capt. Noble said he was a very old observer of Mars, but he had never yet been able to see the extraordinary amount of detail described by some observers now-a-days, and he believed that their esteemed President, the finest astronomical draughtsman of the present generation, did not see the astonishing network of double canals. And it added to the mystery that these details were all observed with comparatively small instruments.

Mr. Holmes said it was not the slightest use to base any conclusions as to the changes on the planets upon drawings made by different people at different times and with different instruments. He would undertake from the Association's Mars reports to show as many changes in one hour as Mr. Griffiths could point out in seven years.

Mr. Griffiths replied that it all depended upon the amount of time given to the observation of the planet.

The President said the point he desired to draw attention to was this: that the planet Mars had upon its surface certain definite tones of shade in which he had seen very little alteration. They were so large and distinct that they could not be missed, and the puzzling thing was that in many of the drawings that exhibited the greatest amount of detail these large shaded markings were not shown.

Mr. Maunder wished Mr. Griffiths had taken a larger section of the planet for his study and compared the drawings. It was pretty clear that what some observers saw as canals other people had seen, in many instances, as the edges of faint markings. With regard to Mr. Griffiths's own drawings, M. Antoniadi, the Director of the Section, described them as among the very best sent in.

Mr. Petrie announced that a set of 70 lantern-slides of pictures relating to the Eclipse Expedition had been purchased for the Lantern-slide Department of the Association.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of this Society on Wednesday evening, February 17th, *Mr. Edward Mawley, F.R.H.S.*, President, read a Report on the Phenological Observations during the past year. He showed that throughout the flowering season, wild plants came into bloom much in advance of their usual time, and were, as a rule, earlier than in any recent year since 1893. The wealth of blossom on nearly all kinds of trees and shrubs was a noteworthy feature of the spring and early summer; while the abundance of wild fruits in the autumn was even more exceptional. From an agricultural and horticultural point of view the one great drawback of the year, which must otherwise have proved one of the most bountiful on record, was a drought that lasted almost without break—at all events so far as vegetation is concerned—from March to September. The wheat crop proved the largest and best for many years, while there was a good yield of barley and potatoes. The small fruits were also good. With these exceptions all the farm and garden crops were more or less indifferent, the crop of hay being especially scanty.

The Hon. Rollo Russell gave the results of some observations on Haze and Transparency which he had made at Haslemere in Surrey. From these it appears that the clearest hours at a good distance from towns are from about noon to 3 P.M. The clearest winds are those from S. to N.W. inclusive, and especially W.S.W., W., and W.N.W.; the haziest are those between N. and E. On bright mornings with a gentle breeze or calm, from autumn to spring, the haze or fog which has lain on the low ground frequently covers the hills in the course of its ascent a few hours after sunrise. At any distance within 100 miles of London, or of the Black Country, observations requiring clear views are likely to be interfered with when the wind blows from their direction, and should therefore be taken early.

The Great Meteoric Shower of November.

It may be safely said that in the month of November, during the next few years, all astronomers and a large majority of the general public will become meteoric observers, for the phenomena presented will be of an exceptional kind and of a character to interest everyone.

There is scarcely any natural event which, to the observant eye, is so brilliant, so impressive and animated, as a rich shower of meteors. A total solar eclipse, with its weird shadow-bands, corona, and red prominences, a large comet, with its train spread over a considerable extent of the heavens, and a vivid Aurora Borealis, with its streamers and condensations of crimson light, have their striking attributes, but it is questionable whether they can compare with the remarkable features which accompany a great fall of shooting-stars. The rarity of the spectacle also enhances its interest, so that when once seen it is never forgotten. For hours meteors descend, not singly or in pairs, but in bursts of 10, 20, or more, and they are mostly fine objects, like Sirius or first-magnitude stars, flashing out here, there, and everywhere in the firmament, and furrowing the sky with rapid flights and often lasting more than a second of time. And it is a special feature of the November meteors that, as the nucleus of each one dies away, an afterglow or streak of phosphorescence comes out along the path; and if these are attentively observed, their directions will be seen to be pretty symmetrical emanations from a focus amongst the stars in the well-known sickle of Leo.

Meteoric observers, and indeed everyone who, either habitually or casually, contemplates the heavens, are extremely fortunate as regards their prospects in the immediate future. There will be rich showers of Leonids during the next 6 or 7 years, not always brilliant; but in the years 1899, 1900, or 1901 there should be magnificent displays, one or two of which will probably be visible in England if the weather is clear. There may also occur a fine exhibition of meteors on about November 23, 1898 or 1899. These meteors are connected with Biela's comet, and radiate from Andromeda: they offered splendid displays in November 1872 and 1885, and it seems fair to assume a recurrence in 1898, a similar interval having passed.

These expected events present a similarity to those actually observed a century ago, when meteoric astronomy was in its early infancy. On December 6, 1798, a brilliant shower of 2000 meteors was observed by Brandes at Leipsig; on November 11, 1799, many thousands of meteors were seen in South America, in Greenland, at Weimar in Germany, and other places, while on April 20, 1803, another shower was observed at Richmond, Va., where the meteors fell too fast to be correctly enumerated, but the estimated hourly number was 800. Thus, within five years, at the close of the last century, three fine meteor-showers were observed, and we shall shortly be entitled to expect a repetition of the phenomena except in regard to the April shower of 1803. The fact that the two other displays alluded to occur at approximately the same years of a century is due to the circumstance that three revolutions of the Leonids (1 = 33¼ years) and fifteeen revolutions of the Andromedes (1 = 6·65 years) are as nearly as possible equivalent to 100 years. Of course, it is not only the end of each century which is thus favoured with meteoric exhibitions, for there are intermediate showers corresponding with the periodical returns of the meteor groups, such as those of the Leonids in 1833 and 1866, and of the Andromedes in 1838, 1872, 1885, and 1892.

It is, however, with the Leonids that we are now immediately concerned. Of all the meteor-showers, old and new, this mid-November stream is the most important. In 1833 it probably formed the most brilliant display ever recorded in our annals. Its history and associations are of extreme interest, and the observations it has provided have assisted in the successful development of the theories of meteoric astronomy more than those of any other system, and a significant feature of it is that a comet is revolving in the same orbit, and that to this comet the meteors owe their parentage. The comet was seen at the end of 1865 and beginning of 1866, and it will probably be reobserved in the spring of 1899. The history of the meteor-stream goes back nearly a thousand years, vaguely and incompletely it is true, but the connecting-link appears to be unmistakable, and the shower of stars in 902 furnishes suggestive evidence, in combination with those of 931, 934, 1002, &c., that it was really the precursor of and identical as regards physical orgin with the showers of 1833 and 1866. We have no details, it is true, of the ancient displays, the record mentioning only the fact and the impression it excited, while it avoids scientific description. Still, such records, bare as they were of salient details, possessed a certain significance and were found valuable inasmuch as the date was given as well as the character of the phenomenon. When compared with modern analogies they materially assisted in determining the true period of the November meteors.

Their history, as already stated, dates from 902, and that our knowledge of this system stretches so far back through the vista of time is due to the praiseworthy efforts made by Herrick, Arago, Quetelet, and Newton to unearth all the records of star-showers from the labyrinth of old historical works. In April 1840 Mr. E. C. Herrick, of New Haven, Connecticut, framed a chronological list of 39 star-showers of former times. In the 'American Journal of Science and Arts' for May 1864 Prof. H. A. Newton gave a catalogue of 13 star-showers presumed to represent different returns of the Leonids as they occurred in the autumn between October 12 and November 12 in the following years:—902, 931, 934, 1002, 1101, 1202, 1366, 1533, 1602, 1698, 1799, 1832, and 1833; and it may be interesting to quote from the descriptions of some of these showers, especially of the three latter and of the subsequent showers which occurred in 1834, 1835, 1836, 1866, &c., &c., where the information is full and supplied by many different observers.

902 October.—On the night of the 12th, when King Ibrahim-Ben-Ahmed died, it is mentioned by Conde in his Arabian history that "an immense number of falling-stars were seen to spread themselves over the face of the sky like rain." The vivid impression produced by this spectacle was long remembered, and the year in which it happened was thereafter known as "the year of stars."

1202 October 19.—It is recorded that "the stars shot hither and thither in the heavens, eastward and westward, and flew against one another like a scattering swarm of locusts to the right and left; this phenomenon lasted until daybreak. People were thrown into consternation, and cried to God the Most High with confused clamour."

1366.—"22 days of the month of October being past, three months before the death of the king, Don Pedro (of Portugal), there was in the heavens a movement of the stars such as men never before saw or heard. At midnight and for some time after all the stars moved from the east to the west, and after being collected together, they began to move, some in one direction, others in another. And afterwards they fell from the sky in such numbers and so thickly together, that as they descended low in the air they seemed large and fiery, and the sky and the air seemed to be in flames, and even the Earth appeared as if ready to take fire. That portion of the sky where there were no stars seemed to be divided into many parts, and this lasted for a long time."

In the old chronicles some national event is usually mentioned as occurring almost contemporary with every celestial phenomenon. With superstition dominant, it will be readily understood that the effect produced by the flight of immense numbers of shooting-stars was a very alarming one, both from its unexpected nature and the mystery involving it. The popular idea was that it was a malign portent, and kings and emperors trembled in insecurity, fearing that these marvellous visitants, like comets and other celestial prodigies, boded change of dynasty or some form of national disaster, such as famine or plague. It is fortunate that the light of science has long since dispelled notions of this character and removed the terrors due to ignorance.

Perhaps no useful purpose will be served by multiplying references to old records. Coming down to modern times it will be found that meteoric showers were described with more regard to scientific detail and accuracy.

Shower of November 11, 1799.

In November 1799 there occurred another great star-shower, and it appears to have been widely observed. Humboldt and Bonpland were travelling in Cumana, South America, and their descriptions of the event must be familiar to astronomical readers. Humboldt said that thousands of bolides (probably) and falling stars appeared during four hours, and that their direction was from north to south. Bonpland related that from the beginning of the phenomenon there was not a space in the firmament equal in extent to three diameters of the Moon which was not filled every instant with bolides and falling stars. All of the meteors left luminous traces or phosphorescent bands behind them. The Guaiqueries in the Indian suburb came out and asserted that the "firework" had begun at one o'clock. The phenomenon ceased by degrees after four o'clock, and the bolides and falling stars became less frequent, but we still distinguished some towards the north-east, a quarter of an hour after sunrise. "The meteors were seen by many of the inhabitants of Cumana, and some of the oldest remembered that the great earthquakes of 1766 were preceded by a similar exhibition. The shower of 1799 was witnessed at many widely-distant stations, for it was reported from North and South America, Greenland, and from several places in Germany. Mr. Ellicott saw it while at sea in the Gulf of Mexico, and has given his impression as follows:—"I was called up at about 3 o'clock in the morning to see the shooting-stars, as they are called. The phenomenon was grand and awful. The whole heavens appeared as if illuminated with sky-rockets, which disappeared only by the light of the sun after daybreak. The meteors, which at any one instant appeared as numerous as the stars, flew in all possible directions except from the Earth, towards which they were all inclined more or less, and some of them descended perpendicularly over the vessel I was in, so that I felt in constant dread of their falling on us."

In 1820, on November 20, shooting-stars were reported to be very numerous, and on the same day of the year 1822, meteors mingled with balls of fire were seen in vast numbers at Potsdam by Klöden.

Showers in November 1833 and neighbouring years.

The *avant-courières* of the brilliant display of 1833 appear to have been noticed as early as November 12, 1830, when Dr. Lardner records that meteors were so numerous as to attract particular notice. In 1831 the shower recurred with increased intensity, and the following account was given to Arago by Captain Berard, of the French brig 'Loiret':—"The 13th of November, at 4 o'clock in the morning, the sky being perfectly cloudless and a copious dew falling, we saw a number of shooting-stars and luminous meteors of great dimensions. During upwards of three hours more than two per minute were seen. One of these meteors, which appeared in the zenith, left an immense train from east to west like a luminous band, and in it many of the colours of the rainbow were distinctly visible; its breadth was equal to one half of the Moon's diameter, and the light it gave did not disappear for six minutes. We were on the coast of Spain near Carthagena."

Dr. Wright, of Ohio, U.S.A., also observed a shower of shooting-stars on the night following November 12, 1831.

In 1832 the display was repeated, and it seems to have been well observed by Captain Hammond at Mocha on the Red Sea, who says, "From one o'clock A.M. till after daylight (November 13) there was a very unusual phenomenon in the heavens. It appeared like meteors bursting in every direction. The sky at the time was clear, the stars and moon bright, with streaks of light and thin white cloud interspersed in the sky. On landing in the morning I enquired of the Arabs if they had noticed the above. They said they had been observing it most of the night. I asked them if ever the like had appeared before. The oldest of them replied that it had not." The shower was noticed at other places, for M. Tharaud, a retired officer of Limoges, stated that, on the night of November 11, 1832, the workmen engaged in laying the foundation of a new bridge over the river Vienne "observed the firmament brilliant with meteors, which at first only amused them, but after some hours the number and splendour of these luminous appearances were so greatly augmented that the people were seized with panics, and so great was their terror that they abandoned their labour and flew to their families, exclaiming that the end of the world had arrived. On the next day these people were interrogated on the subject, and their accounts varied according to the different impressions which had been produced on their imaginations. Some declared that they saw streams of fire, others that they beheld bars of red iron crossing each other in all directions, others that they observed an immense quantity of flying rockets. All agreed that the phenomena were diffused over every part of the firmament, that they commenced at 11 o'clock and continued until 4 the next morning." At Orenburg in Asiatic Russia, on November 13, 1832, between 3 and 4 A.M., a large number of meteors were also seen; between 5 and 6 A.M. the phenomenon reached its greatest splendour and it continued until sunrise.

The display of 1832 was also seen from places in Arabia and from the Mauritius to Switzerland. It was also noticed by the well-known English observer Dawes, who says in Memoirs R.A.S., viii. 76: "1832·865. Most astonishingly brilliant meteors from the east with little intermission, for about an hour, when a thick fog supervened."

In 1833 the shower attained its maximum on November 12, and presented a spectacle, the brilliant character of which it is difficult either to conceive or describe. The terms "prodigious," "stupendous," "magnificent," and "splendid" have been employed to convey an idea of the effect it produced, but mere words must always fail to paint in sufficiently vivid colours the real aspect of a great natural phenomenon. At Boston, U.S.A., the display continued during 7 hours, and it was estimated that 240,000 meteors were visible. The night appears to have been cloudy in England and nothing unusual was seen; but along the whole of the eastern coast of North America, from the Gulf of Mexico to Halifax, the shower was well observed, and the rising sun could not blot out all traces of the phenomenon, for large meteors were seen now and then in full daylight. The meteors were most numerous at about 5 A.M., November 13, when the number falling was considered to equal one-half of the flakes filling the air during an ordinary snowstorm. One observer estimated that during the two hours from 4 to 6 A.M. about 1000 meteors per minute might have been counted. "Within the scope that the eye could contain more than twenty could be seen at a time, shooting in every direction. Not a cloud obscured the broad expanse, and millions of meteors sped their way across it on every point of the compass. Their coruscations were bright, gleamy, and incessant, and they fell thick as the flakes in the early snows of December. One was witnessed which left a path of light that was clearly discernible for more than ten minutes after the ball had exploded. Compared with the splendour of this celestial exhibition, the most brilliant rockets and fireworks of art bore less relation than the twinkling of the most tiny star to the broad glare of the sun."

The shower was witnessed from the West Indies to Canada and from 60° to 100° west longitude. Prof. Kirkwood says that at 10 o'clock in the evening of November 12 shooting-stars were observed with unusual frequency, and that soon after midnight their numbers became so extraordinary as to attract the attention of all who happened to be in the open air. From 2 to 6 A.M. the numbers were so great as to defy all efforts at counting them, while many of them were so brilliant that people sleeping in rooms with undrawn blinds were aroused by their light. In apparent size the meteors varied from half a degree in diameter and rivalling the Moon to the smallest visible points of light. Prof. Kirkwood met an illiterate farmer on the afternoon of the day succeeding the shower, and the latter, after referring to his impressions of the display, remarked that "the stars continued to fall till none were left," and added, "I am anxious to see how the heavens will appear this evening, for I believe we shall see no more stars!" He was evidently ignorant of the very wide distinction between falling stars and fixed stars. Naturally, the great and unexpected fall of meteors created a feeling of consternation among the negroes of South Carolina; and one description of the effect, by a planter, is too impressive to be omitted, though it has often been quoted:—"I was suddenly awakened by the most distressing cries that ever fell on my ears. Shrieks of horror and cries for mercy I could hear from most of the negroes of the three plantations, amounting in all to about 600 or 800.

While earnestly listening for the cause I heard a faint voice near the door calling my name. I arose, and taking my sword, stood at the door. At this moment I heard the same voice still beseeching me to rise, and saying, 'O my God, the world is on fire!' I then opened the door, and it is difficult to say which excited me the most—the awfulness of the spectacle or the distressed cries of the negroes. Upwards of a hundred lay prostrate on the ground—some speechless and some with the bitterest cries, but with their hands upraised, imploring God to save the world and them. The scene was truly awful, for never did rain fall much thicker than the meteors fell towards the earth; east, west, north, and south it was the same." W. F. DENNING.

[To be continued.]

The Orbit of the Comet of A.D. 962.

In the year 1845, the late Dr. J. R. Hind published, in vol. xxiii. (pp. 377, 378) of the 'Astronomische Nachrichten,' the parabolic elements of a comet seen in the year 962. There is no reference to the fundamental data of the calculation except mention of the fact that it was based upon Chinese observations.

Prof. Oudemans, of Leyden, in 1854, while engaged in some researches relative to the comet of that year, took occasion to discuss the observations of the comet of 962, with a view of establishing whether the two comets were identical or not. His results are of interest mainly on account of the statement that he could not reconcile the orbit as calculated by Hind with the observations recorded by the Chinese. On account of this result, Mr. Hind's orbit has always been clouded by suspicion, and I am very glad to state that a recent complete and exhaustive discussion of the Chinese observations and a new calculation of the comet's path have resulted in establishing the general accuracy of the orbit as found by Mr. Hind.

I cannot here enter upon an exposition of all the details, but must content myself with the bare mention of the fundamental data and results. I am bound to state, however, that Prof. Oudemans' deduction of the last observation is quite in error and that he failed to identify the stars composing the stellar group Tsĭ-sing (lunar mansion Sing-Siou), which consist of 7 stars in Hydra.

Of course, the material is extremely crude and besides not over trustworthy; the positions as determined by me may possibly be in error as much as ±5° in Right Ascension, and ±3° in Declination, but the resulting orbit remains in any case pretty near to the values as deduced from the mean of probable positions.

The following are the fundamental data of the calculation:—

Paris M.T.
A.D. 962 Jan. 28·0.... R.A. = 264° 0′ D = + 3° 5′
Feb. 19·0.... R.A.$_{I}$ = 224 30 D$_{I}$ = −11 30
April 2·0.... R.A.$_{II}$ = 140 45 D$_{II}$ = −16 15

The Chinese observations are to be found in the translation by Biot in the 'Connaissance des Temps' for 1846.

The resulting elements are as follows:—

Comet 961 A.D.

T=Dec. 30^{d}·16 Paris M.T.
ω .... 81° 44′ } Equ. & Ecl. 962·0.
☊ 355 40
i 95 26
log q .... 9·72988

For comparison I append the positions probably employed by Hind, and the elements as he has found them:—

A.D. 962.
(I.) Jan. 28·0.... R.A. = 264° D = + 5°
(II.) Feb. 19·0.... R.A.$_{I}$ = 219 D$_{I}$ = − 5
(III.) April 2·0.... R.A.$_{II}$ = 133 D$_{II}$ = − 10

T=A.D. 961 Dec. 30^{d}·16 Greenwich M.T.
π .... 268° 3′ } Equ. & Ecl. 962·0.
☊ 350 35
i 79 33
log q .... 9·7418

Motion retrograde.

The entire work on this as well as other comets will shortly be published at length, and I hope that the discussion of the Chinese observations will prove useful to other investigators.

Turin, Italy. 1897, Feb. 13. GUSTAVE RAVENÉ.

Jupiter's Family of Comets.

EVERYONE who has consulted the list of cometary discoveries in recent years will have noticed the relatively large number of short-period comets which have been added to our knowledge. Since the autumn of 1880 no less than 16 of these interesting bodies have been brought to light if we include the lost comet of De Vico (1844 I.) and the comet of Tempel (1869 III.), the latter of which was not more than suspected to be periodical until its rediscovery in 1880.

I have drawn up the annexed list of comets (p. 131) added to the Jovian family in and since 1880. It will be seen that, with two exceptions, they have perihelia outside the Earth's orbit, their mean distance from the Sun being 1·47, which corresponds to

Year and Number.	Name.	Perihelion Passage.	Periodic Time.	Perihelion Distance.	Inclination.	Next Return.
			years.		° ′	
1880 IV.	Tempel-Swift	1891 Nov. 14	5·534	1·0864	5 23	1897 May 28.
1881 V.	Denning	1881 Sept. 13	8·687	0·7253	6 51	1899 Jan.
1884 I.	Barnard...............	1884 Aug. 16	5·398	1·2798	5 28	1900 Oct.
1884 II.	Wolf	1891 Sept. 3.........	6·821	1·5928	25 15	1898 June 30.
1886 IV.	Brooks	1886 June 6.........	5·595	1·3277	12 43	1897 Aug.
1886 VII.........	Finlay	1893 July 12	6·627	0·9891	3 2	1900 Feb. 26.
1889 V.	Brooks	1896 Nov. 4.........	7·073	1·9502	6 4	1903 Dec.
1889 VI.	Swift	1889 Nov. 29	8·534	1·3537	10 15	1898 June.
1890 VII.........	Spitaler	1890 Oct. 26.........	6·378	1·8179	12 51	1897 March.
1892 III.	Holmes	1892 June 13	6·909	2·1394	20 47	1899 April 27.
1892 V.	Barnard	1892 Dec. 11	6·309	1·4291	31 12	1899 April.
1894 I.	Denning...............	1894 Feb. 9	7·477	1·1494	5 33	1901 Aug. 2.
1894 IV.	De Vico............ E. Swift..............	1894 Oct. 12	5·863	1·3920	2 59	1900 July.
1895 II.	Swift	1895 Aug. 20	7·059	1·2962	3 0	1902 Sept.
1896 V.	Giacobini	1896 Oct. 27	6·549	1·459	11 21	1903 May.
1896 VI.	Perrine	1896 Nov. 24	7·047	1·1127	13 51	1903 Dec.

The elements in several cases are somewhat doubtful, as definitive orbits have not been determined.

136,700,000 miles. The mean inclination of all is 11° 2′, the extremes being Barnard (1892 V.) 31° 12′, and Swift (1895 II.) and Finlay (1886 VII.) about 3°. There seems little doubt that Encke will long retain its position as having the shortest period, for the great majority of these objects recently discovered indicate periods of between six and eight years.

The striking increase effected in the known number of Jovian comets during the last 16 years is remarkable, especially when we remember that, if we omit returns of periodical comets, the average number of cometary discoveries has shown a very slight increase in the period named on the rate prevailing between 1845 and 1879.

While the majority of parabolic comets exhibit perihelia within the Earth's orbit, considerable inclination, and retrograde motion, the elliptical comets of short period have perihelia outside the Earth's orbit, very slight inclination, and direct motion.

Bristol. W. F. DENNING.

Recent Observations of Mars.

IT may be remembered that M. Perrotin, some time Director of the Nice Observatory, recently left that place to associate himself with M. Janssen at Meudon. Here he has been making observations of Mars with the great equatorial $0^{m}\cdot83$ aperture, and has published his observations made last December and January in the 'Comptes Rendus,' vol. cxxiv. no. 7. The following is a translation of the remarks which conclude his memoir:—

' On account of this study, which has enabled me to establish some new features and to verify under different circumstances some results obtained partly at Nice, and more recently upon Mount Mounier during the last six oppositions, it may not be without interest to state precisely in a few words the observational facts which now seem to us to be established without doubt.

1. Considered from the point of view of the general appearance and the colour of the regions which have been seen, it seems that the globe of Mars should be divided into four distinct zones.

These zones, which are of uniform breadth, and of which the contiguous zones encroach upon one another, extend quite round the planet, remaining sensibly parallel to the equator.

Two comprise between them the equatorial regions. The first, much the larger (from 60° to 80° on the average), extends around the northern hemisphere; this is more particularly the zone of the extraordinary canals, the discovery of which we owe to Schiaparelli, and to which we ourselves, by our publications in 1886, called the attention of the astronomical world.

This is also the part of the surface of which the colour, uniformly reddish and strikingly in contrast to that of the other parts, gives to the planet the characteristic colour so well known.

The second zone does not exceed 40° or 45° at its greatest breadth; this is situated almost entirely in the southern hemisphere, and comprises the greater part of the seas. The colour varies from light grey to a very dark grey, nearly black. The continents of this part of the disc not traversed by canals are generally less coloured, less red, clearer and whiter than the continents of the same latitude situated in the region of canals. Certainly, *Hesperia* and *Hellas*, for example, are of a very decided white.

The third and fourth zones, which are bounded respectively by latitude 60° in the northern hemisphere, and by latitude 50° or 60° in the southern, exhibit continents of a white colour or greyish near the seas. Both zones touch the snowy or icy regions round the poles.

2. At the same distances from the centre of the disc the details of the surface are not seen with the *same ease* in the four zones.

Generally the canals do not show with sharpness, except towards the middle of the disc, and the visibility extends further in the direction of the meridians than in that of the parallels. The details of the seas continue to be well distinguished at quite a great distance from the centre, and for the two other zones the visibility is still relatively easy very near the limb, as may be judged from our drawings of 1888. This is notably greater in the neighbourhood of the poles.

3. Except for the regular changes which follow the course of the seasons, and which affect especially the polar ice, the surface of Mars remains invariable in its prominent lines, and transitory changes of detail which we have established with satisfaction are produced in the zone of canals or in that of seas.

In the course of our long study of this planet, two regions have specially been the origin of apparent changes: *Lybia*, the *Lacus Solis*, and their neighbourhoods. The modifications sometimes happening in the canals have not appeared to us to have the regular character given to them by other observers.

4. To these facts of a general kind, we will add two other particulars which arise from our study during this year. The first refers to *Elysium*. This continent situated in the canal-zone has always appeared to us brighter than the parts surrounding, and has constantly given us the idea of relief upon the surface of the disc. This is without doubt only an impression, but it has been so persistent and so often renewed that we are forced to believe that it corresponds to some reality. It will be said that it is a phenomenon of contrast, but we do not think so.

The second fact relates to two seas, depicted in drawing No. 4, and which cross one another without modifying on that account their mutual colours *.

With the luminous projections on the terminator, the bright points of the disc noted by us in 1892, these appearances constitute the riddles of Mars.

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* See M. Perrotin's drawings of 1888, 'Comptes Rendus,' vol. cvii., and Prof. Schiaparelli's map of 1882.

Nevertheless, and in spite of our ignorance of the causes which produce certain of these phenomena, we hope that the preceding summary furnishes information partly new, which is of a nature to extend our knowledge of the configuration of the planet and of the effect of its atmosphere on observations.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Bamfield's Astronomy.

GENTLEMEN,—

I think I have lately come across the most absurd book on Astronomy which was ever written, strong as the expression may seem. The whole title is too long to quote, but the first part is 'A new Treatise of Astronomy, or Astronomy opened.' Lalande mentions it in his 'Bibliographie Astronomique,' but evidently had not seen it, for he not only is uncertain about its date ("1764 ou 1763"), but gives the author's name erroneously, as Banfield instead of Bamfield. Samuel Bamfield tells us that he was a teacher of mathematics at Honiton, Devon; but one hopes that his pupils did not remain with him long. The date of his book is 1764; the copy in the British Museum has a manuscript scrap from the author fastened in, which purports that it was a present from him to some reverend gentleman whose name does not appear. The letter runs thus:—"Honiton, Feb. 20th, 64;—Rev. Sr, As my Book is now published, I desire you w^{d} be pleased to accept of a Copy, and I presume that y^{e} impartial and capable Reader will receive some Pleasure from y^{e} Performance of, R^{d} Sir, y^{r} most obliged humble Servant, S^{m} Bamfield.—Please to let D^{r} Bliss and Mr. Maskelyne have a Sight of it."

Bliss was at that time Astronomer Royal; he died before the year was out and was succeeded by Maskelyne. Bamfield demolishes the Newtonian (or, as he prefers to call it, Keplerian) theory completely to his own satisfaction: what he proposes to substitute may perhaps be worth reading if one wants a little merriment. His suggestions about the motions of the Earth and Moon, and attempt to explain the seasons by the hypothesis of a motion of the Sun round the same centre as the Earth, but in a plane at right angles to that of the Earth's orbit, amount to ludicrous. But his simile to demolish the third law of motion (as he understood, or misunderstood, it) caps the rest. "If any Person," he says, "should give me a full Blow on my Head with a Sledge or otherwise cleave my Head [this word is erased and 'Skull' written over] with a Sword, 'tis plain that either of these Instruments will have a sad Effect on me. But what contrary Effect my Head will have upon either of the Weapons I cannot explain." So he concludes "Sir Isaac's Law hath not the Appearance of either Truth or Reason," and complacently keeps himself clear of "those multiloquous Pedants" who harangue on his wonderful discoveries. In another place he convicts Newton of self-contradiction, because he speaks in one place of the parabolic and in another of the possible elliptic motions of comets; for our learned author contends that an ellipse cannot be a parabola or a parabola an ellipse, and it never seems to occur to him that some comets may move in parabolic and others in elliptic orbits, nor was he mathematician enough, doubtless, to perceive that a parabola is the limiting curve of an ellipse. But he might have given himself the trouble to ascertain easy matters of fact and not write "What Age Sir Isaac was when his Mathematical Principles of Natural Philosophy ushered themselves into the World, I know not." Equally does he show his ignorance in writing "'Tis well known that a great Comet was expected again by all our Astronomers in 1758; but as it did not return, as in 1682, why should we still insist that our Theory of Comets is true?" For surely the fact that Halley's comet *did* return at the of 1758 could as easily have been ascertained as the dates of the birth of Newton and of the publication of the 'Principia.'

Römer's discovery with regard to the motion of light was quite lost upon Bamfield, though it had been confirmed by Bradley's discovery of aberration, which also he rejects and (as he thinks) clearly proves that the propagation of light must be instantaneous, withal expressing his opinion that Bradley was not much of a mathematician, *i. e.*, presumably had not reached the heights which the great Honiton teacher had attained.

As the R. A. S. possesses a copy of this work, its Fellows may entertain themselves with it (if so disposed) at their leisure, as has done Yours faithfully,

Blackheath, 1897, Jan. 27. W. T. Lynn.

Ball's supposed Discovery of the Duplicity of Saturn's Ring.

Gentlemen,—

As Mr. Lynn reverts to Ball's supposed discovery of the duplicity of Saturn's Ring, on p. 98, it may not be wholly out of place if I reiterate here what I pointed out at the time that Mr. L. first invited attention to this question: viz., that the comparatively modern notion that Ball ever saw the chief division in the Saturnian Ring system is due to Kitchiner, who, on p. 354 of his curious Olla-podrida "On Telescopes," published by Whittaker in 1825, speaking of the visibility of this division in some of his own numerous instruments, goes on to say:—"The earliest account which I have met with of an observation of *the Division in Saturn's Ring*, is in the *Phil. Trans. for* 1666, by Mr. Wm. and

Dr. Ball, on October 13th, 1665, at 6 o'clock, with a very good Telescope, near 38 feet long, and a double Eye-glass. This observation has induced the supposition that Saturn is surrounded not by *one* circular body or ring only, but by *two*."

Breen, Smyth, Hind, and others seem subsequently to have blindly copied or followed Kitchiner in this misinterpretation of Ball's observation; and it was not until 1883, when Mr. Lynn and Mr. C. L. Prince thoroughly investigated the origin of the misnomer, "Ball's Division," that it was shown how absolutely untenable any claim to its detection by either of the brothers Ball in reality was.

Yours faithfully,

William Noble.

Forest Lodge, Maresfield, Uckfield.
1897, Feb. 16.

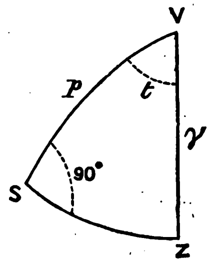
PUBLICATIONS.

Gilliss' Catalogue of Southern Stars. Washington Observations for 1890, Appendix I.—In the year 1849 an expedition under Captain Gilliss was sent by the authorities of the U.S. Naval Observatory to Santiago, for the purpose, among others, of making a catalogue of southern circumpolar stars. The actual programme was to make at least three observations of every star above the eighth magnitude situated between 30° and 90° of south declination; but as this proved impracticable, the work of the expedition was confined to stars situated within 25° of the South Pole, while at the same time the limit of magnitude was extended to what the observers called the tenth magnitude. The instrument used was a Pistor and Martins meridian-circle with an object-glass of 4½ inches aperture; reversible, but it appears that the observations were all made with the instrument in the same position, and with this in the years 1849 to 1852 the observers made about 27,000 observations. On the completion of the observations, Capt. Gilliss was supplied with a staff of computers for the reduction, and the work was pushed on by him with some rapidity; but shortly after his death in 1865 the office was closed and the papers deposited in the U.S. Naval Observatory for safe keeping. For twenty-five years the reduction of the observations was, except for occasional spasmodic efforts, at a standstill, until in 1890 Prof. S. J. Brown, and later Prof. Edgar Frisby, were appointed a board to prepare these observations for press; and, judging from their preface to the volume, this seems to have been no light task. Many discrepancies, mistakes in the earlier work, and ill-advised corrections were brought to light by these gentlemen; but they have produced as the result of their labours a catalogue of 16,748 stars with their places reduced to 1850·0 and the precession in both elements for the same date. Since it appears from a casual glance at the column giving the number of observations that about half the stars have not been observed more than once, and comparatively few more than twice, and after reading of the many difficulties experienced in the reduction, it can scarcely be said that this catalogue is a valuable contribution to accurate astronomy.

STUDIES IN SPHERICAL AND PRACTICAL ASTRONOMY.—Prof. G. Comstock, Director of the Washburn Observatory, has recently published, in the 'Bulletin of the University of Wisconsin,' vol. i. no. 3, a series of short astronomical papers under the above title. These, the author says, consist of an exposition, generally original, of methods for the treatment of certain problems in spherical and practical astronomy, which he has found advantageous in practice. As the characteristic feature of these methods is their brevity, we can hardly hope to make short abstracts of all which would convey intelligible ideas to our readers, but the following extract from one of the papers, which describes a method for determining the angular equivalent of one division of a spirit-level by means of a theodolite, may be taken as an adequate example:—

"Let the spirit-level be firmly attached to a theodolite which is thrown out of level so that its vertical axis makes an angle of from 1° to 3° with the true vertical. It is practically convenient to so attach the level that the radius of curvature drawn through the middle point of its scale shall be approximately parallel to the vertical axis of the theodolite, *i.e.* the level shall be in adjustment. As the theodolite is turned about its vertical axis, the level bubble will run from one end of its tube to the other and back again during a complete revolution of the instrument, and two positions, two readings of the azimuth-circle, may be found in which the bubble will stand near the middle of the scale. A small turning of the instrument either way from one of these positions will produce a corresponding small motion of the bubble in its tube, and this turning of the theodolite and resulting motion of the bubble may be made to furnish not only the value of a division of the level, but also a test of the uniformity of its curvature.

"To determine the relation between the readings of the azimuth-circle of the theodolite and the readings of the level bubble upon its scale, let the accompanying figure represent a portion of the celestial sphere adjacent to the zenith Z, and let V and S be the points in which the axis of the theodolite and the line drawn from the centre of curvature of the level tube through the middle of the bubble respectively intersect the sphere. The arc SV is the intersection with the celestial sphere of a plane passing through S, V, and the centre of curvature of the level tube, and if the adjustment of the level above referred to is approximately made VS may be considered as the intersection with the sphere of the plane in which



the curvature of the level tube lies, so that as the bubble moves in its tube, its successive positions when projected upon the sphere will lie along VS (the angle t varies with the reading of the azimuth-circle), and any position may be identified by its distance from V, represented in the figure by p. Since the bubble always stands at the highest part of the tube, its position S and the corresponding value of p are found by letting fall a perpendicular from the zenith upon the arc VS, and in the right-angled spherical triangle thus formed we have the relation $\tan p = \tan \gamma \cos t$, where γ, as it appears from the figure, is the angle by which the axis of the theodolite is deflected from the true vertical."

It may be readily seen from this formula how the difference between the bubble readings in two positions of the theodolite can be expressed by their equivalent difference of arc read on the azimuth-circle. One constant of the instrument, viz., the reading of the azimuth-circle when the bubble is actually level, can be determined from the fact that there are pairs of positions which give the same bubble-reading. The mean of any pair is the zero-reading sought for. The other constant, viz., the quantity γ, can be determined by means of the vertical circle and a distant object for observation.

Another of Prof. Comstock's papers, on the simplification of the formula for atmospheric refraction, we venture to put in our own words.

In Bessel's expression

$$R = \alpha\beta^{A}\gamma^{\lambda} \tan z,$$

the factors β, γ, $\tan z$ are left unaltered (β being the height of the barometer corrected for temperature, γ being inversely proportional to the temperature reckoned from the absolute zero): the remaining factors being denoted by C . F, where C is a constant, we have

$$\log C + \log F = \log \alpha + (A - 1)\log \beta + (\lambda - 1)\log \gamma,$$

or, with a suitable value for C,

$$\log F = \log \alpha/\alpha_0 + (A - 1)\log \beta + (\lambda - 1)\log \gamma/\gamma_0,$$

α_0, γ_0 being the values of α, γ corresponding respectively to zero zenith distance, and a convenient value of the temperature. Log α/α_0 and $\lambda - 1$ may be treated as proportional to $\tan^2 z$, $\log \gamma/\gamma_0$ to the difference between the actual temperature and the above-assumed convenient value. $A - 1$ is considered negligible. We have therefore,

$$R = C . F\beta\gamma \tan z,$$

$$\log F = (\mu + \nu . T)\tan^2 z,$$

μ, ν being small negative constants, and T the temperature. For zenith distances less than 75°, no sensible error is introduced.

The above two examples are ingenious, and show the style of these papers. There are others on correcting the Sun's declination for refraction, on the determination of flexure and inequality of pivots of a "broken" transit, and on the determination of Time or Azimuth from transits over the vertical of the Pole Star, all of which are interesting and appeal more especially to those engaged on longitude and survey work.

ANNALS OF THE STRASBURG OBSERVATORY, VOL. I.—In 1872, soon after the Province of Strasburg had become part of the German Empire, Kaiser Wilhelm I. determined to attach an observatory to the university of that place. Dr. F. A. Winnecke was appointed Director, and the building, begun in 1877, was completed in 1882. The first volume, which is issued by Dr. Becker the present Director, gives a description of the building, with a picture showing it to be a structure quite entitled to the epithet "Royal," and details of the instruments. The observations given in the volume are the results of the daily work with the Repsold meridian-circle from 1882 to 1886, the right ascensions and declinations being given on the same page. There are also added a few pages of meteorological observations in the years 1873–1879.

NOTES.

COMET NOTES.—M. Ristenpart has computed new elliptical elements of Comet *g* 1896 (Perrine, Dec. 8), making use of 27 observations made in December and January (Ast. Nach. 3402):—

T = 1896 Nov. $24^{d}\cdot6567$ Berlin M.T.

ω	$163^{\circ}\ 53'\ 30''$	1897·0
☊	$246\ \ 34\ \ 36$	
i	$13\ \ 40\ \ 26$	
ϕ	$42\ \ 47\ \ 17$	
log e	9·832054	
log q	0·045405	
μ	$550''\cdot901$	
a	3·4617	
Period	$6^{y}\cdot441$	

The comet cannot be actually identical with that of Biela, but the two may very probably be both fragments of a single original comet. This partition may possibly have taken place in 1845, when Biela's comet is known to have split into at least two fragments; but a more accurate knowledge of the period of the new comet is required before this question can be decided with certainty.

The following ephemeris is for Berlin midnight:—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Mar. 4....	6 22 33	3 31	Mar. 20....	6 54 38	4 39
8....	6 30 46	3 51	24....	7 2 23	4 51
12....	6 38 50	4 9	28....	7 10 1	5 2
16....	6 46 47	4 25	April 1....	7 17 33	5 11

Ast. Nach. 3401 contains some further researches by H. J. Zwiess on the orbit of Comet Holmes (1892 III.). His previous elements are, however, very slightly modified: he determines that the comet will next be in perihelion 1899 April 27·675, which only differs by 7 hours from his previous determination.

The 'English Mechanic' for Jan. 15 contains a letter from Mr. Swift, in which he gives some further details concerning the two objects seen by him in the neighbourhood of the Sun on Sept. 20 and 21 last. He reiterates his conviction of their cometary nature.

The 'Bulletin Astronomique' for January contains elements and ephemerides for Comets Tempel-Swift (1869 III.–1880 IV.) and d'Arrest, both of which pass perihelion this year. These are by MM. J. Bossert and G. Leveau. Perturbations have been applied in the first but not in the second. There is little chance of seeing Comet Tempel-Swift this year, for even when brightest (on May 31) it will only be $\frac{1}{8}$ as bright as when last seen in 1892. D'Arrest's Comet, on the other hand, when at its brightest (on June 2), will be twice as bright as when last seen in 1890; its visibility this year is therefore quite probable.

	Tempel-Swift.		d'Arrest.	
Epoch..	1897 June 8 Paris M.T.		1897 Jan. 1·5 Paris M.T.	
T	1897, June 4^{d}·739		1897 May 21^{d}·7	
π......	43° 26′ 41″	1900·0.	319° 26′	1897·0.
☊	296 27 10		146 21	
i......	5 23 25		15 43	
φ......	40 42 37		38 51	

Ephemeris for Tempel-Swift.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Apr. 29....	1	13	49	10	54	June 8....	3	59	23	23	19
May 9....	1	52	13	14	40	18....	4	43	40	24	46
19....	2	32	52	18	8	28....	5	27	17	25	24
29....	3	15	28	21	4	July 8....	6	9	13	25	15

Ephemeris for d'Arrest.

	R.A.			Dec.			R.A.			Dec.	
	h	m	s	°	′		h	m	s	°	′
Mar. 6..	19	40	13	10	12 S.	May 25..	0	15	10	2	38 N.
16..	20	12	37	9	8	June 4..	0	48	51	4	4
26..	20	46	0	7	47	14..	1	21	16	5	12
Apr. 5..	21	20	13	6	13	24..	1	52	8	6	2
15..	21	55	0	4	29	July 4..	2	21	8	6	32
25..	22	30	13	2	38	14..	2	48	1	6	41
May 5..	23	5	32	0	47 S.	24..	3	12	31	6	30
15..	23	40	37	1	0 N.	Aug. 3..	3	34	23	5	59 N.

Ast. Nach. 3402 contains the following ephemeris of Comet 1889 V. (Brooks) for Berlin midnight. The brightness on March 8 is only one-fifth of that at rediscovery:—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	°
Feb. 28....	1 43 50	13 26	Mar. 8....	1 59 57	15 3
Mar. 4....	1 51 52	14 15	12....	2 8 5	15 50

A. C. D. C.

MINOR PLANET NOTES.—The following planets have received permanent numbers:—

CV (Wolf, 1896 Sept. 3)..	418	CZ	421
CW	419	DA (Witt, 1896 Oct. 8) ..	422
CY	420		

CU, CX do not receive permanent numbers, not having been sufficiently observed.

The following planets have been named:—

412....	Elizabetha.	420....	Bertholda.
413....	Edburga.	421....	Zähringia.

A. C. D. C.

THE VARIATION OF LATITUDE.—In our number of 1895, March, we republished from the 'Astronomical Journal' Dr. Chandler's most recent formulæ for determining the variation of latitude at any place, and a diagram which exhibited this for any epoch during the period from 1890·0 to 1895·5. In a recent number of the A. J., No. 392, he publishes a table giving data sufficient to compute the variations for the present year. If ϕ be the actual latitude, and ϕ_0 the mean latitude of a place in longitude λ west of Greenwich, the variation at any time is given by $\phi-\phi_0=x\sin l-y\cos l$, where x and y are taken from the following table:—

1897.		x.	y.	1897.		x.	y.
Jan.	4	+0″·019	+0″·159	July	23	−0″·025	−0″·193
	24	·093	·174	Aug.	12	·101	·209
Feb.	13	·157	·171	Sept.	1	·166	·204
Mar.	5	·208	·154		21	·215	·178
	25	·237	·117	Oct.	11	·242	·133
April	14	·244	·067		31	·244	·073
May	4	·226	+0 ·009	Nov.	20	·223	−0 ·006
	24	·186	−0 ·053	Dec.	10	·178	+0 ·065
June	13	·126	·110		30	·115	+ ·128
July	3	+ ·054	−0 ·159		50	−0 ·042	+0 ·182

It may be remembered that the quantities x and y are the coordinates of the pole of figure with respect to the pole of rotation as origin, the positive direction of the axis of y passing through Greenwich.

Dr. Chandler's formulæ are based on observations made previous to 1894·0, and therefore a comparison of the observations of latitude made since that date with the latitude computed by his formula supplies a test of the accuracy of the latter. "Such a comparison, using a normal observed series of $\phi - \phi_0$, fifty days apart from 1894·0 to 1896·5, based on the combined observations by Talcott's method at five European stations, gives an average difference, without regard to sign, of only ±0″·041; which is sensibly not greater than the uncertainty of the observed points themselves." Dr. Chandler then goes on to promise a paper shortly on the accuracy of his formulæ, and adds: "I permit myself to mention here that a discussion of the whole series of observations from 1889 to 1896·5 demonstrates that the radius r_1, of the 428-day revolution has been diminishing in accordance with the law of equation (52) announced in A. J. 322, and indeed at a slightly greater rate. It is now only 0″·12, whereas at the beginning of the decade it was 0″·19."

THE ROTATION OF JUPITER.—Prof. Oudemans communicates to Ast. Nach. no. 3401 the results of some observations of the spots on Jupiter made with the Utrecht refractor of 26 cm. aperture by M. Nyland, to determine the rotation period in different Jovian latitudes. The results of the observations are given (we omit details of the dates of the observations, but the results refer to epochs in the first three months of 1896) in the following table:—

Latitude.	Rotation period.	Latitude.	Rotation period.
°	h m s	°	h m s
−33	9 56 24·5	−10	9 55 7·8
−27	55 44·3	− 9	55 25·7
−15	55 1·2	+11·7	55 30·9
−12·5	54 59·9	+12·0	55 34·3
−11	55 9·3	+23·5	56 1·6
		+28	56 0·3

The result given second in the table is from observation of the red spot.

THE COMPANIONS OF SIRIUS AND PROCYON.—Some recent observations by Prof. Schaeberle, given in the 'A. J.,' No. 394, are as follows:—

	Date.	Pos.-ang.	Dist.
Sirius	1896·824	188°·3	3″·65
	1896·830	190 ·0	3 ·65
	1896·849	188 ·3	3 ·85
	1897·025	189 ·4	3 ·72
Procyon ..	1896·868	318 ·8	4 ·58
	1896·871	320 ·4	4 ·58
	1896·966	319 ·3	4 ·89
	1897·025	322 ·3	4 ·62

DR. SEE is working energetically at the discovery of double stars at Mexico City, and evidently means to make the most of the wide unexplored regions of the southern sky.

Here are some of his finds.

α Phœnicis, 13th mag.	265° and	7″
π Phœnicis, 13th mag.	255	6
μ Velorum, 11th mag.	60	2·5
η Centauri, 13·5 mag.	270	8
β Lupi, mags. 3 and 3·5	90	0·25

He also finds that 𝔥 3683 has a rapid motion and has performed 1½ revolutions since Herschel observed it. This star ought to be attended to.

A STAR-ATLAS *.—Messrs. Gunn and Co., of New York, have sent us a copy of a recently published star-atlas by Prof. Winslow Upton of Brown University, of which we can speak in praise. This is designed for educational purposes, and is therefore of small size, 8½ by 14 inches, which makes it handy for the study table. It contains six maps, two showing respectively the northern and southern circumpolar constellations as far as 40° from the poles, and the intermediate zone from −40° to +40° is divided into four and shown on the other maps. On these, as the work is intended for the use of teachers, the ancient figures which give their names to the constellations are shown in faint outline. Besides the six maps just described, these are also reproduced on a smaller scale, with only the brighter stars shown and these joined by lines, which seems to us to be a good aid to learning the constellations by sight. The introductory text gives a considerable amount of information about the nomenclature of stars and the constellations and also convenient lists of variable and double stars. Anyone buying his first telescope might do worse than to buy this book with it.

THE effect of this Diamond Jubilee year is making itself felt even in astronomy. In the February number of 'Knowledge' Miss Clerke has written one of her admirable historical essays on the progress of astronomy during the reign of Her Majesty, illustrated by pictures of Airy, Adams, and Sir John Herschel, and of the typical equatorial telescope of 1837, in contrast with the elaborate eye-end of the Lick telescope. In reading the article one is at first struck with the fact that the progress of the science has been almost wholly in spectroscopy, and in what is known now as physical astronomy; but on further consideration one notices that little mention is made of such things as the geometry of the solar spots, the variation of latitude, and other researches in geometrical astronomy, which, though equally meritorious, are not so sensational and do not make such an effect on the popular mind as the discovery of a comet or the photograph of a nebula. Even the circumstances of the discovery of Neptune make a very poor show in a magazine article now-a-days.

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* Star-Atlas containing stars visible to the naked eye, and clusters, &c., visible in small telescopes, and an explanatory text by Winslow Upton, A.M.

WRITING of physical astronomy reminds us of an address delivered by Prof. Mendenhall before the Philosophical Society of Washington when he retired from the Presidential chair in 1892, January. This address, entitled, "On the Uncertainty of Conclusions," has lately been published in vol. xii. of the Society's Transactions and is full of interest. Speaking of the exactness of the conclusions of astronomy, he pointed out that this same exactness resulted from the fact, not that the logic of the astronomer was simpler than that of anyone engaged in any other science, but rather that the premises were simpler; in fact, they deal only with *one* property of matter, namely, its attractive power; but he went on to say: "Within the last twenty-five years, however, the splendid discovery of spectroscopy, aided by great improvements in photography, has given rise to a new astronomy known as physical, as distinguished from gravitational astronomy. The new science deals with a matter of many properties, some of which are but little understood. While its conclusions are of vital importance and of intense interest, they result from deductions in which the premises are insufficient and are proportionately uncertain. The new astronomy must for a long time abound in contradictions and controversies until, and largely through its development, we shall possess a knowledge of the properties of matter when subjected to conditions differing enormously from those which we are now quite familiar. Because one astronomer declares that the temperature of the Sun is 20,000° F., and another, equally honest and capable, says that it is not less than 20,000,000° F., it must not be inferred, and it never is except by the superficial, that the whole science of solar energy is a tissue of falsehoods, and that those engaged in its development are planning an imposition upon the general public. Even such widely varying results as these may be based on observations that are entirely correct and experiments that are beyond criticism. The discussion of the results obtained by observation and experiment may follow in both cases the very best models, and yet the conclusions may be erroneous and contradictory owing to the insufficiency of data in the beginning."

WEATHER CHARACTERISTICS OF 1896.—Brief remarks on the leading features of the weather in London in the past year have appeared in the 'Observatory' from time to time as occasion required, and it may now be sufficient to advert to a few of the salient points which have not hitherto been touched upon. The monthly mean temperatures, as determined at the Royal Observatory, Greenwich, from January to July, were all in excess, as compared with the 50 years' averages, to the mean amount of 2°·4, whilst the remaining months were usually below average values by small amounts. The mean annual temperature was 50°·1, being

$0^\circ.7$ above the average. Rain fell on 161 days, which is about an average number, whilst the year's rainfall, 22·42 inches, is only 2·12 inches below its corresponding average, the liberal downpour in September amply compensating for the droughts which marked the earlier part of the year. In the 20 years 1877–1896 there is a total deficiency of rainfall, as compared with the average, amounting to rather more than 5 inches. With regard to the wind,—a preponderance of winds from N. and N.E. was experienced in May, of N. again in August, and of N.E. in November, whilst the S.W. wind preponderated in March, September, and October and was almost entirely absent in May. In the year the N. wind prevailed on 64 days and the W. wind on 61 days, and these numbers are respectively 21 days and 18 days above the usual degrees of prevalence. The S.W. wind with 88 days and the S. wind with 28 days are, on the contrary, respectively 17 and 13 days below the average amounts. W. C. N.

PROF. GIOVANNI V. SCHIAPARELLI, the Director of the Royal Brera Observatory in Milan, has been recently elected a Foreign Member of the Royal Society. Prof. Schiaparelli's list of work is probably known to every astronomer, but it may not be out of place to recapitulate it here. His earlier papers, published about the year 1867, were on the subject of meteors and comets. "The Relation that exists between Comets and Shooting-stars," "On the Origin of Shooting-stars," "On the Origin of the November Meteors," appeared in the 'Astronomische Nachrichten.' He has more recently devoted himself to observation of the features of the surface of the planets, and was the first to see the so-called canals on Mars. From his observations of Venus, he deduced that the time of the planet's rotation was about 225 days, thus agreeing with the period of her rotation round the Sun, which contradicted for the first time the period of about a day which had held its place since the time of the younger Cassini. Schiaparelli's period has since been confirmed by other observers, and is now probably considered to be entitled to as much weight as the shorter period by the majority of astronomers. He has deduced similarly for Mercury a period of 88 days, agreeing with the sidereal period.

SOME ASTRONOMICAL PRIZES.—In connection with the Exhibition to be held in Brussels this year, the Belgian Government offer money prizes (the first three are of the value of 900 francs, 600 francs, and 500 francs respectively) for the solution of certain problems in astronomy. The subjects of the problems (these are accurately given in 'Nature' of Feb. 18) include the following:—The value of *g*, the cause of the Martian canals, the photography of planetary details, the motion of the solar system, the variation of latitude, the secular acceleration of the Moon, and a method for observing the corona without an eclipse. If this pecuniary reward does induce anyone to solve this last problem, or in fact any of the others, we think the money will be well spent.

The Royal Meteorological Society proposes to commemorate the Diamond Jubilee by an exhibition of meteorological instruments in use in 1837 and in 1897, and of diagrams, drawings, and photographs illustrative thereof. The exhibition will be held at the Institution of Civil Engineers from March 16th to 19th.

We call the attention of any astronomer who is thinking of settling himself in a new abode, to the fact that the observatory of the late Mr. George Hunt is going begging. Or rather that to anyone who will buy her house, Mrs. Hunt will present the observatory. An announcement will be found in our advertisement pages.

M. Gaillot has been appointed Sous-Directeur of the Paris Observatory in succession to M. Loewy, who is now Director.

From an Oxford Note-Book.

Just as we were going to press a month ago, a telegram was received from Professor E. E. Barnard, announcing that he had started in the 'Trave' from New York for Southampton. The 'Trave' was expected on Tuesday, February 9, and we felt justified in announcing that the Gold Medallist of the year would honour the Royal Astronomical Society by appearing in person to receive the Medal on Friday, February 12; and we all prepared a hearty welcome for one who showed so keen an appreciation of this recognition as to cross the Atlantic in the winter in order to be present on the great occasion. But alas! we reckoned without that same wintry Atlantic. On Friday morning nothing had been heard of the 'Trave,' and the North-German-Lloyd people could only surmise that the weather had been very rough, or that the dense fog off the Needles was keeping her from getting in to Southampton. This state of suspense naturally took the keen edge off the meeting and the dinner which followed it, though a very pleasant evening was passed. The unsatisfactory nature of the situation was emphasized by the receipt of a kindly telegram from Dr. Oudemans, addressed jointly to the Society and to Professor Barnard, to which we had not the heart to reply at the time.

Fortunately our suspense was not prolonged much further. Our Medallist arrived at Southampton early next morning, and was at Burlington House by 10.30 a.m. on Saturday. He had *not* enjoyed himself, and was clearly ready to appreciate a little rest and quiet. We were sorry to learn that he could not stay until March 12; and as there may be many who would like to see him, an informal meeting of the R.A.S. has been arranged for Tuesday, March 2, at 4.30 p.m., with a dinner, as nearly as possible like that of February 12, to follow.

"If there are any new Fellows present, now is the time to be admitted." This announcement is made at each Meeting of the R. Ast. Soc., and one or two usually advance to the President's table to grasp the right hand of Fellowship. It is an agitating moment, and probably few, if any, of these ever notice a bruise on the corner of the table to which they advance. This bruise is historical. It was made by Dr. Gill's heliometer when it came down with a crash ten days before it was to be taken to Ascension to observe the Opposition of Mars in 1877. The story of the cruel catastrophe is told in Mrs. Gill's charming book, 'Six Months in Ascension' (John Murray, 1878):—

> "The instrument had never been used in so low a latitude as Ascension, and it was necessary to test it carefully in order to ascertain whether it would perform its functions well under the untried circumstances.
>
> "Considerable interest in the expedition having been shown by Members of the Royal Astronomical Society, it was thought best to erect the instrument in their rooms at Burlington House, where the necessary trials could be made, and that the instrument might afterwards be exhibited and explained at one of the evening meetings. The heliometer was duly erected and all had been brought nearly into the same condition of affairs as would be required at Ascension. David was applying a level to an inclined piece of wood cut to the angle of the latitude of Ascension, and was directing the workmen to give a final motion to the screw by which the inclination of the axis is changed, when slip! the screw gave out, the overhanging weight of the Heliometer and its counterpoises tore the lower end of the cradle from his hand, and, tilting upwards, the polar axis, counterpoise, weights, and Heliometer tube, in all several cwt., came down crash from a height of 7 or 8 feet upon the floor.
>
> "Imagine the astronomer's feelings as he saw the Heliometer of all his hopes light upon its delicate eye-end; that eye-end driven through the floor and slowly torn off as the whole mass gradually turned round, smashing and crashing the more delicate rods, handles, and other attachments to the tube, and finally squashing one of the copper caps which protect the ends of the slides from dust."

By dint of great exertions the heliometer was repaired in time and the situation saved. As Mrs. Gill remarks, had the precaution of setting up the instrument in England not been adopted, the accident might have happened in Ascension, when nothing could have been done. I wonder whether accounts of accidents to scientific expeditions would make an interesting collection? It falls to Dr. Gill's lot to mention a more serious accident in the introduction to the recently-published 'Report on the Geodetic Survey of South Africa, 1894.' As many of our readers may not have seen this work, I extract verbatim:—

> "Nothing further was done in South Africa in the way of Geodetic Survey, or even of extensive accurate triangulation, till 1859, when a triangulation of the south-eastern part of the Cape Colony was undertaken, partly for the purpose of forming reference points to correct the then very inaccurate and defective state of the charts of the coast, and partly to afford means of better connecting the detached property surveys throughout that part of the Colony. The work was entrusted to Capt. Bailey, R.E., aided by one sergeant and 13 rank and file of the R.E., five of whom were selected from the Ordnance Survey of England. The cost of this work was borne by the colony. When in 1862 the work was completed, the survey party embarked at Algoa Bay

en route for England in the coasting-steamer 'Waldensian.' The vessel struck upon the rocks off Struy's Point and became a total wreck. On board were the instruments, drawings, original observation-books, and sheets with full abstracts, calculation-books of every kind, all complete in every respect. These were all lost and have never been recovered. Fortunately copies of 'abstracts of angles' had been supplied to the Admiralty Surveyor engaged on the coast survey, other abstracts of angles with a diagram to the Surveyor-General in Cape Town; abstracts of angles and certified copies of the original observations for 17 stations had also been sent to the Government of British Kaffraria, and from these and sundry copies and abstracts sent to private individuals or surveyors, an account of the work was compiled by Capt. Bailey and printed in a report presented to the Cape Parliament in 1863."

TALKING of expeditions reminds me that the marks of the recent eclipse expedition are not easily shaken off completely. Among the properties which cling to one the baggage-labels are perhaps the most persistent. I have not taken any very strong measures to remove them from my Gladstone-bag, and so they remain there and occasionally excite remark. The other day they produced a monologue of some length, to the following effect: "Been travelling lately? Not lately? Ah! I was in Paris a week ago. Paris is getting very gay. Now there's the Café ——. Funny performance that is. There are no lights, and the whole place is like a funeral. You are given a place near a coffin, and pay 65 cents for a drink and a small taper. Then when enough people assemble you are taken into a small theatre, where there is a coffin standing upright. They get one of the company to stand in it, and I suppose they have some joke like Pepper's ghost, for you see him decompose and finally become a skeleton. Very funny! Oh! Paris is gayer than ever it was!"

A SENTENCE in one of our less serious contemporaries has puzzled me more than a little. It treats the "plagiarism of the ancients" from quite a new point of view. This plagiarism is just as trying in science as in literature, and almost more common. There are probably very few of us who have not had at least once the aggravation of finding that some idea which we had hoped was new had occurred to others long ago; and, with all our elaborate machinery of catalogues and indexes, we do not find this out until too late. It is somewhat startling, therefore, to find that in the case of jokes the difficulty of making sure of originality is treated lightly. The sentence referred to reads as follows:—"The following jokes.... have been adjudged the best sent in this week, and Postal Orders for 10*s.*, 7*s.* 6*d.*, and 2*s.* 6*d.* will be forwarded to the authors *if within a week they are found to be original.*" We thus get two superior limits to the magnitude of the enterprise of testing the originality of three jokes: it can be completed within a week, and the labour involved is worth less than twenty shillings altogether. I wonder what the figures would be in the case of three average astronomical papers?

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MEETINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

Tuesday, March 2, 1897.

Sir R. S. Ball, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., and
H. F. Newall, M.A.

An informal Meeting was held at 4.30 P.M., in accordance with the following postcard which had been circulated:—

> ROYAL ASTRONOMICAL SOCIETY.
> Burlington House, London, W.
>
> Our Medallist, Prof. E. E. Barnard, who hoped to have been present at the Annual Meeting, arrived at Southampton at 2 A.M. on the following morning, his ship having been delayed four days by very bad weather. He must return before the next Meeting of the Society, on March 12.
>
> Under the circumstances the President and Secretaries suggest an informal Meeting of the Society at 4.30 on Tuesday, March 2, when Prof. Barnard will exhibit some of his slides.
>
> H. F. Newall } *Sec etaries.*
> H. H. Turner }
>
> A Dinner has also been arranged to follow the Meeting, at the Criterion Restaurant, Piccadilly Circus, with the same arrangements as on February 12, viz.: Morning dress. Price 7s. 6d., exclusive of wine. *Intending diners are requested to send a postcard to Mr. Wesley.*

The Meeting was very well attended, the room being practically quite full. Professor Barnard gave a most interesting exhibition of his slides, of which the following is a brief account.

He first showed a beautiful picture of "the old Moon in the new Moon's arms," photographed in thirty seconds; all the detail of the dark part of the Moon illuminated by earth-light was most beautifully shown. Then followed a photograph of the partially eclipsed moon taken with the 12-in. visual telescope of the Lick Observatory, the image being wonderfully sharp for such a telescope; and another of the totally eclipsed Moon on September 3, 1895, taken at mid-totality with seven minutes' exposure. Then followed some of his own drawings of the planet Mars. Professor Barnard stated that he was unable with the Lick 36-in. telescope, even under very favourable conditions, to see the canals as depicted by other observers. And he added that the markings on Venus had never been seen by him as described by Mr. Lowell. The conditions for observing during the daytime at Mount Hamilton were not favourable, the unequal heat of the Sun on mountain slopes making the definition generally bad. But on one occasion, 1889 April 26, the atmosphere being very thick from distant forest fires, the definition was simply perfect; and then, although he could see no fine narrow markings on Venus, he had seen well some of the large broad markings which had been drawn by other observers. It appeared, however, that Mr. Lowell did not see these large broad markings, although he saw and drew small thin ones. Prof. Barnard was at some pains to insist that he was in no way questioning Mr. Lowell's observations, but simply recording his own. He then showed about a dozen cometary pictures, including those of Holmes' comet, Brooks' comet 1893, Swift's comet 1892 April, Gale's comet, and the comet he had himself discovered by photography. He drew particular attention to the wonderful changes in the tail of Brooks' comet shown by the photographs taken on three consecutive days, 1893 October 21, 22, 23, and remarked that these changes seem suggestive of an encounter with some resisting medium. But he only made this suggestion in the most tentative manner. Then he placed on the screen several of his wonderful pictures of the Milky Way taken with the Willard lens. In connection with the picture of the star-cloud in Sagittarius (R.A. $18^h 10^m$, Dec. $-20°$) he drew attention to two intensely black holes sharply defined in the star-cloud, from which run two semi-vacant lanes of stars, and these after leaving the star-cloud seem to be continued as lines of small stars of uniform size, like beads on a thread. With reference to the picture of the great Nebula of Antares, discovered with the Willard lens on March 23, 1895, he called attention to the fact that this Nebula condenses strongly about ρ Ophiuchi and 22 Scorpii and another star, and is connected by a nebulous extension with the bright star σ Scorpii. Hence he considered that these stars are at the same distance from us as the Nebula, which is seen on examination to be connected with the vacant lanes and the small stars forming the ground-work of the Milky Way, and he concluded that these small stars are really small compared with our Sun, and not simply more distant than others. Other pictures showed cases where the Nebula was not condensed about bright stars. By an ingenious arrangement of a mask to cover one of the slides, Prof. Barnard showed how very small was the field of the Lick telescope available at any one moment compared with that covered by a photograph. Finally, he showed two pictures of the Yerkes Observatory, one of them showing its actual state at present, the great dome being erected but not covered, and the other (the architect's drawing) showing it as it is intended to be.

The slides and remarks met with a very hearty reception from the Fellows present.

The President invited comments.

The Astronomer Royal said that he had been much interested in the new fields of inquiry opened up by the work of Prof. Barnard. He had been struck by the fact that his photographs were in many respects better *pictures* of the heavens than those taken in connection with the scheme for the Astrographic Chart, which showed only comparatively small portions at a time. He might mention that he had had the opportunity of comparing a plate of the Præsepe taken at Greenwich with the 13-inch object-glass with forty minutes' exposure with one taken with the Willard lens with sixty minutes' exposure. The two photographs showed as nearly as possible the same number of stars, which was noteworthy, considering that the former was taken with a 13-in. lens, and the latter with a 6-in. Pictures of both kinds had their own special uses, but he had been very much impressed, nevertheless, by the valuable results obtained by Prof. Barnard.

Prof. Turner remarked on the great importance of having the maker of these pictures present with them to give his own interpretation of the results. He could not but feel a little sceptical as to that most fundamental suggestion concerning the size of the stars in the Milky Way, which Prof. Barnard had explained to them. It did not seem certain that what appeared from the photograph to be the case might be finally accepted as a reality without independent evidence. But in any case the conclusion was one worthy of the closest attention.

Dr. Johnstone Stoney suggested with regard to the changes in the comet's tail, that it might have met, not perhaps with a resisting medium, but with a meteor-stream. There must be countless such meteor-streams in space, judging from the number which our small Earth moving in its small orbit encounters every year, and it did not seem unlikely that this would account for the shattering of the tail which they had seen on the screen. He further suggested the necessity of varying the exposure in photographs. He did not think we should realise the facts completely until all possible exposures, short and long, had been made. He had been particularly struck by the slide showing the smallness of the field of the Lick telescope as compared with the photograph, and suggested that this explained the faulty character of drawings of the Andromeda Nebula.

The Rev. S. Kinns expressed the intense pleasure he had felt in listening to the lecturer. When Prof. Barnard had been present in 1893 he had heard him with such pleasure, and had gone away in such ecstacies, that he could not sleep all night. He was correspondingly disappointed on the occasion of the Annual Meeting when he heard that Prof. Barnard would not be there. Then he had received the postcard announcing that after all there would be an opportunity of hearing him, and he had been so delighted that he had read the card over and over again.

Prof. A. S. Herschel called particular attention to the meteor-trail shown on one of the comet pictures, and congratulated the lecturer on having secured it, especially in view of recent failures to photograph the November Meteors on this side of the Atlantic. He had had the pleasure of seeing a photograph taken in San Francisco of a display of fireworks, and he understood that Prof. Barnard had photographed several of the August Meteors on a single plate, and these, he thought, gave us hope for the future. With reference to the comet caught by its trail on the plate, perhaps this kind of discovery might be repeated in the case of Tempel's comet and the November Meteor comet.

Mr. E. W. Maunder had particularly admired the picture of the "old Moon in the new Moon's arms." He considered it eloquent testimony to Prof. Barnard's skill as a photographer that he should be able to obtain such beautiful detail of the dark part, side by side with such a bright object as the crescent, and such skill as was therein shown doubtless explained much of his great successes in other directions. Further, he would remark upon the demonstration which Prof. Barnard's beautiful photographs had afforded of the necessity for using all classes of instruments in studying the structure of the universe.

The President expressed the great pleasure and enjoyment he had had in hearing Prof. Barnard, and considered that they had all obtained much food for reflection. He was afraid that henceforward he should regard the canals in Mars with some suspicion, nay, even the seas had partly fallen under a ban. Perhaps the lecturer's recent experiences on the Atlantic might explain something of this mistrust. [Laughter.] With regard to the comets' tails and the marvellous representation of the disasters they meet with, we could hardly refuse to accept the suggested explanation (possibly in the form adopted by Dr. Stoney); but the point which was perhaps most important was that concerning the small stars in the Milky Way. He was not inclined at the present moment to adopt the sceptical attitude taken up by the Secretary. He would prefer to allow himself the liberty of accepting the most important suggestion of Prof. Barnard, which merited the greatest possible attention. He had realized for the first time the two classes of Nebulæ which had been pointed out to them: one kind apparently mixed up with stars, the other not. And he regarded as one of the most mysterious objects he had ever seen, the collection of two or three small holes seen on one of the photographs of Nebulæ. Had it not been for Prof. Barnard's well-known skill as a photographer we might have suspected that these spots had some accidental origin, but, as it was, they were most enigmatical. In conclusion he was sure he represented every person present in returning most hearty thanks to Prof. Barnard for the very interesting address he had given them. [Applause.]

The dinner in the evening was also very well attended, thirty-seven being present.

The President made a remarkably good speech in proposing the toast of the evening, the health of Prof. Barnard. He dwelt upon his wonderful achievement in discovering the fifth satellite of Jupiter, which had immortalized both himself and the Lick telescope. If that telescope were dismounted to-morrow it would still have done enough to take classical rank. It was an old saying that men were known by their friends, and it was an honour to Prof. Barnard that his most intimate friend was our Medallist of three years ago, Mr. S. W. Burnham, just as it was an equal honour to Mr. Burnham that he should be the friend of Barnard. They welcomed Prof. Barnard there that evening in the name of the whole Society; and more than that, surely they with their Royal Charter might claim to represent the astronomical science of England, and ask the American nation to recognize in the award made to Prof. Barnard the general appreciation of one nation for the work done in the other [applause].

Prof. Barnard in reply asked them to take for granted much of what he would say in gratitude for the kind and hearty reception they had accorded him. That he was present on this occasion was chiefly due to Mrs. Barnard, who was herself an Englishwoman [applause] and who had insisted that on this great honour being done him he should come and receive the Medal in person. He had always felt a great admiration for the work that had been done by English astronomers, and this admiration had been much increased during the last few weeks when he had had experience of the climate in which they worked [laughter], and seen some of the comparatively humble instruments. He had especially admired the work of Dr. Common, who had practically originated all this work of photographing Nebulæ, which had been followed up also in England by Dr. Roberts and others. He was afraid that in America they were scarcely yet enthusiastic enough to make or even do much work with the reflecting telescope, but they were all the more ready to appreciate the work done with it in England, and he would ask permission to conclude by proposing the health of the retiring President, Dr. Common.

Dr. Common hoped that on retiring from the Presidential Chair he had got rid of the necessity for making speeches. He remarked on the number of medals which had lately been awarded to Americans, and said that when he was preparing the address last year on Dr. Chandler's work on the variation of latitude it had occurred to him whether this variation might not be due to the disturbance caused by shifting so much gold from England to America. [Laughter.]

Prof. Turner proposed the health of Mrs. Barnard, a toast which was received with great enthusiasm.

The Astronomer Royal proposed the toast of "The Visitors," connecting with it the name of Sir George Baden-Powell, M.P., who, in replying, commented on the comparatively small amount of public money spent on science in England, a circumstance which he considered called for alteration.

Meeting on Friday, 1897 March 12.

Sir R. S. Ball, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., and H. F. Newall, M.A.

The Minutes of the last ordinary Meeting were read and confirmed.

Mr. Newall. Since the date of the last ordinary Meeting a large number of presents, amounting to one hundred and thirty-seven, have been received by the Society. I will call attention to some which are included in the list. There is a book by Prof. Comstock, presented by the Washburn Observatory, entitled 'Studies in Practical Astronomy,' which will be found of interest; copies of new editions of Mr. Lynn's three books, 'Celestial Motions,' 'Remarkable Comets,' and 'Remarkable Eclipses'; a book by Dr. See on the 'Evolution of Stellar Systems'; a report by M. Kostinsky of the observations of the total Solar Eclipse of 1896 August 9, made by the Russian Expedition in Novaya Zemlya, with lantern-slides made from photographs of the corona obtained by this party, which will be shown on the screen. We have also a fine series of large positives on glass of portions of the Moon's surface, and transparencies of the total solar eclipse of 1889 December, presented by the Lick Observatory. Besides these I have to announce the receipt of valuable presents from the Executors of the late Mr. Sidney Waters, including a number of books and an equatorially mounted camera with a tele-photographic lens which Mr. Waters had designed for observation in Norway of the recent eclipse.

The photographs of the solar eclipse taken by the Russian party above mentioned were shown on the screen.

Mr. Wesley. The larger photographs were taken by M. Kostinsky, in Novaya Zemlya, with a 4-in. photographic telescope and exposures of 3 secs. and 10 secs. The small photograph was taken by M. Hansky with a small camera and an exposure of over 20 secs. I have carefully measured the length of the greatest extension of the corona, and can trace it to a length equal to 2½ diameters of the

Moon. This is in the north-east quadrant. This spot M. Hansky believes to be the planet Jupiter, and I think that this is the first time that the planet Jupiter has been photographed on the same plate with the corona.

Captain Noble. What is the orientation of these pictures?

Mr. Wesley. They are placed with north pole of the Sun uppermost as nearly as the plates will allow.

Dr. Spitta. What is the description of the plates?

Mr. Wesley. I do not think anything is said about the make of the plates. I think that appearance (pointing to screen) looks like a thin cloud, because it has a distinct form. On looking at the entire plate with a strong electric light in the background the whole image appears to be clouded.

Mr. W. E. Wilson then showed some photographs of the Spiral Nebula which he had taken recently with his 2-foot reflector.

Capt. Noble. Comparing these with the representations of the same nebula which were shown by Dr. Roberts on the screen a few months ago, it strikes me that Mr. Wilson's photographs show the individual stars in the spiral infinitely more distinctly than Dr. Roberts's pictures. Those who were present will remember that attention was invited to the stars, and to the way in which they followed the curve of the nebula, but they were considerably more nebulous than these. Mr. Wilson's are very much more stellar than those shown in Dr. Roberts's photographs.

The President. To me these beautiful photographs shown by Mr. Wilson are of particular interest from the circumstance that the Observatory in Ireland is no great distance from Lord Rosse's at Parsonstown, where this nebula was first discovered, and I am pleased to ask you to congratulate Mr. Wilson upon the excellence of these specimens of his skill.

Prof. Turner. It will probably be news to most of those present that there is, within 100 miles of London, a portrait-lens of 15 inches aperture and 89 inches focus, which takes plates such as I hold in my hand. Dr. Spitta thinks he can presently show them on the screen. The observatory has, among other luxuries, a hand-power rising-floor. Both instrument and floor were made by Sir Howard Grubb. Their owner prefers to remain unnamed, but I have his permission to show these photographs. His first plate was exposed on 1896 January 19, the doublet having been finally mounted some days before.

Photographs of the regions near M 56, the Pleiades, γ Cygni, ξ Cygni, M 81–82, and 6 Comæ were then thrown on the screen, and excited much admiration. The delineation of the nebulæ in the Pleiades close to the bright stars was particularly noticeable.

Captain Noble. These pictures show the advantage of a refractor over a reflector for photographing stars. Those who have studied the images of stars shown by a reflector will remember the appearance of an unsightly cross, which is not at all apparent on these.

The President. We must all be pleased to hear of this anonymous astronomer who is capable of doing such excellent work as we have had the pleasure of seeing, but looking at the size of his pictures it seems to me that it will be a question for the lantern committee whether we shall not have to provide a 15-inch lantern to do justice to the photographs.

Rev. W. Sidgreaves (having been called upon to read his paper on β Lyræ). As there are many papers on the card for the evening, I will only draw attention to two or three points in my paper. I propose to explain in more detail, and with the aid of a diagram, the interpretation of the radiation-line λ 4472, which I gave at the January Meeting, and which was reported in the February number of the 'Observatory.' [Father Sidgreaves proceeded to explain his diagram, which showed the variation of wave-length of different lines with the phase of the period.]

With reference to the tabulated positions of the spectrum lines accompanying the paper, I must state that many of them will be found entered as *probable*, but I have no hesitation in adding that these may be prudently accepted as *certain*; the distinction between a certain and a probable line being that the latter admitted of itself the interpretation of contrast-effect, and was decided by its appearance on other plates, while the former could not be explained away.

Finally, I call attention to a marked difference of behaviour observed in the two bright hydrogen-lines Hβ and Hγ. The former is always strong, and is certainly at its fullest intensity before the chief minimum, while Hβ faded continuously between each maximum and the following minimum phase, and recovered abruptly shortly after each minimum. A careful re-examination of the best plates showed that this discordance between the two hydrogen lines could not be attributed to the difference between the photographic sensibility at the two parts of the spectrum; there was no appearance of over-exposure at the positions of Hγ.

Mr. Newall. I have been greatly interested at the immense amount of work and study that Father Sidgreaves has devoted to this spectrum, and I would ask him if he has dealt with the behaviour of the various lines according to the elements from which they originate. I was struck with the similarity in the behaviour of the hydrogen lines Hγ and Hδ. The variation from the normal position of the lines observed is shown to be very similar in the case of these two lines, but not so in the case of Hβ. One would look to find a difference between the helium lines attributable to the separate hypothetical constituents of the cleveite gases. I would ask Father Sidgreaves if he has paid special attention to that point.

Rev. W. Sidgreaves. I have not dealt with that special point.

Mr. Newall. You have dealt with each line on its own merits, and if there was radiation of certain bright lines you have followed the variation of wave-length of those lines?

Rev. W. Sidgreaves. I have simply dealt with the lines as they are, without reference to their origin in any way. That I leave to you.

Prof. Turner explained the principles of a proposed Photographic Transit-Circle. The advantages claimed for this over the usual form of instrument were (1) that it would supply a means of eliminating personal equation, and (2) that it would eliminate difficulties due to irregular refraction. The apparatus consisted of a fixed horizontal photographic telescope placed in the meridian. The plate in the camera was carried by clockwork across the field, its rate of progress being recorded by an intermittent spot of light controlled by the observing clock which marked the plate, and hence the plate fulfilled the function of the chronograph sheet. The other essential part of the apparatus was a plane mirror in front of the object-glass, supported on an axis in an east-and-west direction, like the axis of an ordinary transit-circle. A star being reflected into the telescope, and the plate exposed, the image more or less elongated according to the relative rate of motion of the star and plate, but this elongation is immaterial, would be formed on the plate, and the position of this image with respect to the marks denoting the time, and also with respect to a mark showing the line of collimation, could be measured and the time of transit deduced.

Mr. Dyson. Professor Turner's suggestion for this new form of instrument is very interesting to me. So far as I understand it, the mirror is really the transit-circle, and requires the same adjustments, but I am not clear whether the moving plate or the position of the lamps does not bring in one additional adjustment. I should like to ask Prof. Turner whether an additional adjustment is not required for the mirror. I was a little surprised to hear Prof. Turner talk of giving so long an exposure. It seems to me that the instrument would be adapted for a shorter exposure of 10 or 20 seconds.

Prof. Turner. Mr. Dyson has asked me if a new adjustment is introduced. There is one new adjustment, but I do not think it is a serious one, because it is a matter of detail. The new adjustment is part of the collimation, which is made up of about three different parts. There is first the inclination of the mirror to the axis. It must be parallel to its horizontal axis. As to the zenith-distance observations, the circle is placed on the mirror and the microscopes read in the usual way. The advantages gained in zenith-distance observations are mainly under the head of irregular refraction. The great gain is that this mirror can be placed out in the open, and the rays of light from all stars go as far as the mirror without disturbance from being inside the observing-room. I think there is considerable advantage in having the mirror in the open air, and there will also be some elimination of flexure.

These are the chief gains with regard to zenith-distance observations. With regard to the question of exposure I think that ordinary stars, such as clock-stars and the polar star, would be photographed in a second or two. My notion of working the instrument would be to have adjustments determined once in the evening, and to have one long series of plates.

The President. We are very much obliged to Prof. Turner. It seems that photography is going to invade the one corner of astronomy which has hitherto been reserved for the old method, but we can only hope to see this experiment tried.

Dr. Downing. I have thought that it is not too early to begin to collect meteorological observations to make a forecast of probably favourable stations for observing the total solar eclipse of next January; and so long ago as 1893 I wrote to Mr. Eliot, the head of the Meteorological Department in India, asking him to secure observations at stations which lie in the path of the Moon's shadow in 1898. Mr. Eliot at once acceded to my request, and was able to collect observations made in January of the years 1894, 1895, 1896, and 1897, and in fact with the greatest kindness took a great deal of trouble to see that very complete series of observations were secured. What I asked for was the amount of cloud visible in the sky at intervals from 11 to 3 Indian time during the latter half of January. The time of totality in India is about 1 o'clock in the afternoon, and the day of the eclipse is January 22. I recently received the record for January 1897, and I immediately proceeded to get the observations into shape to present them to the Society in the hope that they might be useful to observers. I think the statistics give sufficient information for the purposes of the astronomer, but they can only be rough indications, and I will give the rough results. Starting from the West Coast of India, where the line of totality strikes the coast near 73° E. longitude, I find the average amount of cloud in the sky for 73° to 76° E. long. at that time of year and day is represented by 1 (10 being a clouded sky). From 78° to 85° E. long., the average amount of cloud in the sky may be represented by 2. The most easterly place for which I have returns is Darjeeling, and there the amount of cloud rises to between 7 and 8. The moral of the discussion is sufficiently obvious—that to the west of the range of mountains from Ahmadnuggar to Hyderabad, the probable amount of cloud may be represented by the ratio 1 : 10; to the east of the mountains by 2 : 10. It would be advisable, therefore, for the observer not to go further east than longitude 85°, and it would appear that he has a better chance of a clear sky if he keeps to the west of the range of mountains.

A Fellow. Is there any difference in the amount of cloud on the low-lying coast-line between the sea and the Ghauts and on the hill-stations?

Dr. Downing. I am afraid there is not a station on the hills. Sholapore is 76° E. long., and is a station in the plain the furthest east of the first set referred to, but in that neighbourhood the ratio 1 : 10, representing the probable cloud, is very uniform.

The President. I am sure we are all very much interested in this communication, which is very encouraging to those who are going to try and see the Indian Eclipse. I would ask Dr. Downing why he did not get similar particulars for us with regard to the last eclipse. I am very glad, however, that he did not, otherwise we might not have had a very delightful trip.

Dr. Downing then read a paper on the discordances of the index errors of the Madras mural circle 1834–42.

Prof. Turner read extracts from a paper by *Prof. E. W. Brown* on the mean motion of the lunar perigee and node, and also a paper by the same gentleman on theoretical values of the secular accelerations in the lunar theory.

Mr. Cowell. Two years ago Prof. Brown wrote a very important paper upon these subjects, and produced results of great analytical interest, but on trial I found that I could not apply Prof. Brown's methods without using very much more labour of computation than would be necessary by the ordinary methods, and I therefore came to the conclusion that Prof. Brown's paper, though of great analytical interest, was hardly suitable for numerical calculations. I mentioned this to Prof. Brown, and he told me that he hoped to be able to arrange his formulæ so as to make them more suitable for numerical calculation. I shall therefore look forward with great interest to the publication of this paper.

Mr. Newall read a paper by *Mr. Lowell* on further proof of the rotation period of Venus.

Mr. Stone made some remarks on the present magnitude of Nova Aurigæ, which was observed at the Radcliffe Observatory on March 10 last to be of magnitude 11·5, and on March 12 of magnitude 11·0. Previous observations in 1894 and 1895 had shown its magnitude to be constant about 9·7.

Mr. Newbegin showed on the screen some photographs of recent sun-spots.

The following papers were announced and partly read:—

E. W. Brown. "On the Mean Motions of the Lunar Perigee and Node."

E. W. Brown. "On the Theoretical Values of the Secular Accelerations in the Lunar Theory."

Royal Observatory, Cape of Good Hope. "New Double Stars found at the Cape Observatory in 1896."

Royal Observatory, Edinburgh. "Observations of Comets and Planets."

W. Coleman. "Micrometrical Measures of Double Stars."

T. K. Mellor. "The Nuclei of a Sun-spot."

T. J. J. See. "New Short-period Binary in Dorado."

S. W. Burnham. "The Orbit of δ Cygni."

Mrs. Young (*Miss G. Chisholm*). "On the Curve

$$y = \left\{ \frac{1}{x^2 + \sin^2\psi} \right\}^{\frac{3}{2}}$$

and its connection with an Astronomical Problem."

A. M. W. Downing. "Discordances of Index Errors of the Madras Mural Circle, 1834–42."

H. H. Turner. "On a Photographic Transit-Circle."

P. Lowell. "Further Proof of the Rotation-Period of Venus."

Rev. W. Sidgreaves. "The Spectrum of β Lyræ."

"Observations at Vadsö during the Total Solar Eclipse of August 9, 1896, by Passengers of the s.s. 'Neptune.' Communicated by *Rev. T. C. Porter.*

A. M. W. Downing. "Cloud Statistics referring to the Total Solar Eclipse of 1898, Jan. 21–22, in India."

A. Marth. "Ephemeris for Physical Observations of Mercury."

E. J. Stone. "Estimations of the Magnitude of Nova Aurigæ made at the Radcliffe Observatory, Oxford."

Radcliffe Observatory, Oxford. "Transit-Circle Observations of Comet Swift (1896 April 13) *sub polo.*"

Isaac Roberts. "Attempt to Photograph the Leonids in the Open Sky."

The following gentleman was elected a Fellow of the Society:—

J. Harold Herbert, Mapperley Road, Nottingham.

The following Candidates were proposed for election as Fellows of the Society:—

A. W. Claydon, M.A., Principal of the Exeter Technical and University Extension College (proposed by Wm. Ellis).

H. L. N. Noel-Cox, 2 Edinburgh Mansions, Victoria Street, S.W. (proposed by Richard Inwards).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fourth ordinary meeting of the current session was held at University College, on Wednesday, 24th February, the President, *Mr. N. E. Green, F.R.A.S.*, in the Chair.

Eight new members were elected, and the names of four candidates for election were read and suspended.

Mr. Seabroke described by the aid of the blackboard a mechanical arrangement for correcting the observed time of transit of a celestial object for the three principal errors, viz., collimation, level, and azimuth.

Mr. A. J. S. Adams read a paper on "The Development and Decline of Sun-spots," illustrated by diagrams. Mr. Adams pointed out that these diagrams gave apparently some support to Mr. Corder's theory that sun-spots wax and wane under the Earth's influence, notwithstanding that the known laws of gravitation and physics would appear to make such a theory impossible. Nevertheless, the apparent remarkable preference of sun-spot growth for one or another solar quadrant, together with the possibility of some law being traceable regarding increase and decay, seemed to render a further examination of the question desirable. Mr. Adams then proceeded to describe the method he had adopted for arriving at the facts. The results appeared to indicate that there was a sudden increase of area as the spot apparently moved from the limb, and the question arose: Was this a physical truth, or was it the result of error in measurement or in the applied correction? The writer was inclined to think that the phenomenon was less real than it looked, and that very careful consideration should be given to it before its acceptance. Certainly the difficulties of observation near the limb were sufficient to create a doubt. But supposing that the results were reliable, there would seem to be no way out of the question but by admitting the truth of Mr. Corder's theory. In conducting the enquiry regard had been paid to the frequency of area expansion upon one side or the other of the solar central meridian, and it was interesting to note that the increased areas were found frequently to occupy a space confined more or less to one or another quadrant. This feature was amply demonstrated by a simple inspection of the Greenwich "results," and while he thought that an Earth-influence was extremely improbable, cumulative evidence might yet bring to light the existence of some solar tidal progression, which threw these disturbances into groups of more or less rhythmic form.

Mr. E. W. Maunder said that he was exceedingly pleased that Mr. Adams had taken up this inquiry; his only regret was that the areas of the umbræ had been taken rather than of the whole spots, because he thought that it was perfectly clear that much of the falling off which was shown towards either limb depended upon the difficulty of observing an umbra when it was near the limb. Taking the mean curve which Mr. Adams had given below the annual curves as representing the general effect obtained for the measures of some seven or eight years, it would apparently imply that the umbra of a spot was very small when it came into view at the East limb, that when it had advanced some 30° (or three days' journey) on to the Sun it there reached its maximum size, and that it remained with very little change until it got within three days of the west limb, when it declined very rapidly. He thought they could have no difficulty in agreeing with Mr. Adams that a considerable amount of that deficiency in the umbra near the limb was apparent only and was not sufficient evidence of terrestrial influence. But the figures which Mr. Adams had given in his second table seemed far more definitely in favour of a real Earth effect in the development of spots, and would show that on the whole they were very perceptibly larger on the east side of the central meridian than they were on the west. That was certainly the impression which many striking groups had given him in a casual observation; but it was only by the treatment of a great number of observations over a very large extent of time that they could be quite sure they were not being led away by a mere casual appearance.

Mr. G. J. Newbegin exhibited some slides of sun-spots covering the period from January down to the present time.

Mr. H. Dennis Taylor then delivered a lecture upon "The Evolution of the Telescope." The lecturer, after alluding to the first vague hints at optical contrivances for aiding the eye in the 'Opus Magnus' and other works of Roger Bacon, traced the development of the telescope from its invention by Hans Lippershey, a spectacle-maker of Middleburg in Holland, in 1608, to the present time. The lecture was illustrated by lantern-slides of the more noted telescopes, taken in many cases from old engravings and drawings. A hearty vote of thanks to the lecturer concluded the meeting.

ROYAL METEOROLOGICAL SOCIETY.

This Society has for many years past held annually an Exhibition devoted to some special class of meteorological instruments.

This year the Society arranged an Exhibition of meteorological instruments in use in 1837 and 1897, in commemoration of the Diamond Jubilee of H.M. the Queen. The Exhibition, which was opened on Tuesday the 16th instant, was held in the Large Library of the Institution of Civil Engineers, Great George Street, Westminster, S.W.

The instruments which were in use in 1837, as might be supposed, are not very numerous, but many of them were somewhat quaint and of great interest. Sir E. H. Verney, Bart., showed an old barometer with a large spirit thermometer, which latter has an arbitrary scale decreasing as the temperature increases, "extreme cold" being 90° and "extreme hot" 0°.

The instruments in use during 1897 were, however, very numerous, and comprised various forms of barometers, thermometers, hygrometers, rain-guages, anemometers, nephoscopes, sunshine recorders, actinometers, aneroids, electrical and miscellaneous instruments. Many of the instruments which were self-recording were shown in action.

An interesting exhibit was a railed-off enclosure about 12 feet square covered with green baize, representing a typical Climatological Station of the Royal Meteorological Society. This included a Stevenson thermometer screen, fitted with dry bulb, wet bulb, maximum and minimum thermometers; rain-gauge; solar and terrestrial radiation thermometers; sunshine recorder; and earth thermometer, all of which were placed *in situ*. The Exhibition also included a number of charts and photographs of great interest, particularly those by Mr. J. Leadbeater of ice crystals on window-panes. Mr. W. H. Dines showed an experiment illustrating the formation of the tornado cloud, and Mr. Birt Acres exhibited some exceedingly interesting studies of form and movement of clouds and waves, projected on the screen by his cinematoscope. The Exhibition remained open until Friday, March 19th.

In connection with the exhibition, a lecture was delivered on Wednesday evening, March 17th, by *Mr. G. J. Symons, F.R.S.*, on "Meteorological Observations in 1837 and 1897." After describing some of the instruments in use at the commencement of the Queen's reign, the lecturer stated that he had collected all the known records of rainfall for the year 1837, and that he was able to give the total rainfall for that year from 161 stations. An account was then given of the meteorological instruments in use at the present day, reference being made to the barometer, thermometer, hygrometer, rain-gauges, anemometers, self-recording instruments, &c. The methods adopted for registering the duration of sunshine and the amount of evaporation were next described. Allusion was also made to the help which photography has rendered to meteorology, especially in relation to the forms and movement of clouds, &c. Mr. Symons concluded his lecture by exhibiting a map, showing the state of the weather over Western Europe at 8 A.M. the same morning. The lecture throughout was illustrated by numerous lantern-slides, and *Mr. Birt Acres* exhibited his cinematoscope photographs mentioned above.

The Great Meteoric Shower of November.

[Continued from p. 129.]

DESCRIPTIONS of the fine shower which attracted so much attention in 1833 might be multiplied, but without useful purpose. Lardner says the phenomenon was witnessed from the Atlantic Ocean to Central Mexico, and from the North-American Lakes to the southern side of the Island of Jamaica. "Everywhere within these limits the first appearance was that of fireworks of the most imposing grandeur, covering the entire vault of heaven with myriads of fireballs resembling sky-rockets. The meteors exhibited, on closer inspection, three distinct varieties—the first consisting of phosphoric lines, apparently described by a point; the second of large fireballs, that at intervals darted along the sky leaving numerous trains which occasionally remained in view for a number of minutes, and in some cases for half an hour or more; the third of undefined luminous bodies which remained nearly stationary for a long time."

During this brilliant display the fact that the meteors radiated from a common centre in the head of Leo was distinctly manifested, and the position of this centre was accurately determined by Olmsted and Aiken.

In 1834 November 12, Prof. A. C. Twining, of West Point, N.Y., observed many meteors from Leo. They were also noticed to be very numerous at other places in America.

1835 November 13.—At Amenia, N.Y., the hourly rate was determined at 40 or 50. An unusual number of shooting-stars was recorded at several other American stations. Sir J. Herschel witnessed a tolerably fine display at the Cape of Good Hope on the night of Nov. 13.

1836 November 12.—Mr. Dunster, Springvale, Maine, U.S.A., saw 253 meteors in a watch extending over 2¾ hours. 500 were seen at Randolph College, Va., and 400 at Newark, A.I.

1837 November 12.—Prof. Olmsted reported 226 meteors seen by eight observers at New Haven between $13^h\ 5^m$ and sunrise. At another station 52 were counted by one person during 1¼ hour.

1838 November 12.—Mr. R. C. Woods, of London, in a letter to the 'Times' relates that from $15^h\ 25^m$ to $15^h\ 55^m$ "nothing could exceed the grandeur of the heavens. From E.N.E. to N. meteors fell like a shower of bomb-shells in a bombardment, and in such rapid succession as to defy every attempt to watch their particular directions or to ascertain their numbers. At $15^h\ 55^m$ the shower ceased." Mr. Woods estimated that he saw 400 or 500 meteors during the half an hour. On November 13, in 4¼ hours, 233 meteors were counted by eight observers at Cambridge, Mass.

1839 November 12, $7^h\ 28^m$ to $12^h\ 7^m$.—120 meteors were recorded by Heis at Aachen.

These several showers, though representing well-defined returns of the Leonids, were incomparable with the splendid display of 1833. They proved, however, that the meteors returned annually, though the maxima might be separated by intervals corresponding to the period between 1799 and 1833.

The phenomenon of 1833 may be said to have heralded the dawn of meteoric astronomy. The event was so striking, and its cosmical associations apparently so important, that astronomers directed earnest attention to the subject of meteors: old records were ransacked for descriptions of past showers, and the various observations of the displays of 1799 and 1833 were carefully compared and investigated. The material collected proved useful, though it was singularly incomplete and inexact. This department of the science had been systematically neglected in former years, meteors having been generally regarded as atmospheric bodies which played no important *rôle* in the economy of the solar system. Prof. Olmsted discussed the phenomena recorded in 1833, and some of his results were well founded, for he concluded that the meteors originated beyond our atmosphere, that they consisted of light, transparent, combustible materials, which took fire and were consumed in passing through the air, and that they formed portions of a nebulous body which revolved round the Sun. Dr. Olbers, in 1837, assumed the meteors to be moving in cometary or very eccentric ellipses, and he pointed out the periodicity of the shower, predicting at the same time a probably brilliant return in 1867. In 1840 Mr. E. C. Herrick gave as the most plausible hypothesis "that there are revolving around the Sun millions of small planetary and nebulous bodies of various magnitudes and densities and that when any of these dart through our atmosphere they become ignited and are seen as shooting stars."

Showers of 1866 *and neighbouring years.*

After 1839 the Leonid shower seems to have virtually disappeared, but, as time drew near its expected recurrence in 1866 or 1867, the interest revived. To say that the display fully answered expectation, if it did not rival that of 1833, is but to tell an old story. The results of some observations may perhaps be most conveniently summarized in a tabular form (p. 166).

The finest display was seen in England in 1866, when 100 meteors per minute were visible to one observer at about the time of maximum, which occurred at $13^h\ 10^m$, but showers of considerable brilliancy were also observed in 1867 and 1868 in America. In 1867 the weather was cloudy in England, but in 1868 a display of fine meteors was seen at Greenwich on November 13 after 14^h, though clouds greatly impeded observations.

In addition to the figures given in the table, it seems desirable to quote two descriptions referring to abundant showers of Leonids in 1864 and 1865.

1864 November 13, A.M., s.s. 'Ellora,' off Malta.—"There was a grand display of meteors all through the watch. The watch, an old 'salt' and an intelligent man, said that it was the grandest display he had ever seen, and that the whole watch had been looking at them with delight." No meteors were visible on the morning of November 14.

1865 November 12.—Mr. William Black, Master of the British ship 'Compeer,' writes:—"I have to inform you that on the night of Sunday, November 12, while passing Redonda and Nevis in the West Indies, on my way to Mobile, the heavens were in a blaze with shooting-stars from 8 P.M. till 5 next morning, such as I had never witnessed before in upwards of 30 years while in the command of a ship. The wind was light at the time, from S.E., temp. 82°."

After 1869, the shower seems to have become so far exhausted as to have presented no conspicuous features. It, however, continued to furnish a few meteors every year at the middle of November.

Simultaneously, with the return of the Leonids, meteoric astronomy made rapid progress. In 1864 Prof. H. A. Newton

Date.	Hours of observation.	Meteors seen.	Time of Maximum.	Number of Observations.	Observer and Place.
	h m to h m		h m		
1860 Nov. 12	10 0 to 16 0	381	14 30	5	D. Kirkwood, Bloomington, Ind.
1861 12	15 0 to 17 0	130	16 30	4	A. C. Twining, New Haven, Conn.
1862 13	12 0 to 15 0	59	......	2	S. J. Gummere, Hareford Coll., Pa.
1863 13	10 10 to 17 7	213	15 0	6	J. M. Gilliss, Washington.
" 13	10 38 to 17 16	316	15 30	7	S. J. Gummere, Haverford Coll., Pa.
" 13	13 0 to 17 20	97	15 30	1	B. V. Marsh, Germantown, Pa.
1864 12	13 45 to 16 0	46	......	1	R. H. Stretch, Nevada.
1865 12	12 0 to 17 0	279	13-14	6	Greenwich Observatory.
" 12	13 0 to 14 0	77	......	1	O. B. Wheeler, Detroit, Mich.
" 12	15 0 to 16 0	63	......	1	O. B. Wheeler, Detroit, Mich.
1866 12	12 0 to 17 0	603	15 30	10	C. S. Lyman, New Haven, Conn.
" 13	11 0 to 14 0	492	13 0	10	C. S. Lyman, New Haven, Conn.
" 13	6 0 to 17 0	10602	14 15	4	J. F. J. Schmidt, Athens.
" 13	9 0 to 17 0	8786	......	8	Greenwich Observatory.
" 13	11 54 to 14 40	5600	......	2	F. Howlett, Canterbury.
" 13	11 0 to 18 0	3087	13 24	2	R. Main, Oxford.
" 13	12 0 to 13 7	1634	13 11	4	J. R. Hind, London.
" 13	11 15 to 16 50	8000*	13 12	1	G. J. Symons, London.
1867 13	14 30 to 16 45	1900	......	3	Capt. Stuart, Nassau.
" 13	12 0 to 18 0	2287	16 14	4	G. T. Kingston, Toronto, Can.
" 13	10 0 to 16 0	4937	......	10	N. R. Leonard, Iowa.
" 13	12 0 to 17 45	3730	15 50	Many.	Prof T. H. Safford, Chicago.
" 13	13 0 to 18 0	3044	16 30	Several.	J. McClure, Philadelphia.
" 13	13 0 to 16 28	1474	16 22	15	H. A. Newton, New Haven, Conn.
1868 13	10 45 to 18 0	2886	16 30	2	G. T. Kingston, Toronto, Can.
" 13	12 0 to 17 54	5573	17 27	10	H. A. Newton, New Haven, Conn.
" 13	11 34 to 17 40	5000	17 0	...	S. J. Gummere, Haverford Coll., Pa.
" 13	....	3766	14 30	5	Maria Mitchell, Vassar Coll., N.Y.
" 13	14 20 to 17 58	4278	16 55	4	J. R. Eastman, Washington.
" 13	12 51 to 17 15	5670	17 10	...	C. G. Rockwood, Brunswick, Me.
" 13	14 30 to 17 45	2204	17 22	3	P. Secchi, Rome.
1869 13	15 20 to 16 40	428	16 5	3	C. Meldrum, Mauritius.
" 13	14 30 to 17 14	136	14 30	1	G. L. Tupman, Port Said, Egypt.
" 13	7 0 to 19 0	830	......	4	Observed at Fredericton, N.B.
" 13	13 18 to 15 43	556	......	2	G. Davidson, Santa Barbara, Cal.

* Number estimated.

Local times are given in all cases.

published a list of previous displays, and these, with the Earth's longitude on each occasion and the shower of 1866 added for comparison, are given by the late Prof. J. C. Adams as follows in 'Monthly Notices,' xxvii. p. 248:—

Year.	Day and hour.	Earth's longitude.
902	October 12, 17h	24° 17′
931	14, 10	25 57
934	13, 17	25 33
1002	14, 10	26 45
1101	16, 17	30 2
1202	18, 14	32 25
1366	22, 17	37 48
1533	24, 14	41 12
1602	27, 10 O.S.	44 19
1698	November 8, 17 N.S.	47 21
1799	11, 21	50 2
1832	12, 16	50 49
1833	12, 22	50 49
1866	13, 13	51 28

Prof. Newton, from his examination of the subject, decided that the meteors "had a motion along the sidereal year of one day in 70 years, and that the shower had a period of about a third of a century." He found that the intersecting point of the Earth and meteor-cloud exhibited a mean annual motion of 52″·4 with respect to the fixed stars. This corresponds to 29′ during the 33¼ years separating the maximum returns. Prof. Newton also pointed out that the group must have a periodic time of revolution of either 180·0, 185·4, 354·6, 376·6 days, or 33·25 years, and stated that the true period might be ascertained by calculating the amount of disturbance due to planetary attraction. This difficult work was undertaken by a mathematician well qualified to perform it, and with the result that the latter period was found to be the correct one. Prof. Adams found that during 33¼ years the longitude of the node is increased 20′ by the action of Jupiter, nearly 7′ by that of Saturn, while Uranus is responsible for 1′, making 28′ altogether, and, almost coincident with the 29′ proved by observation.

Thus the periodic time received settlement. But this was not all, for another remarkable development was effected at about the same epoch. Prof. G. V. Schiaparelli, of Milan, who had been occupying himself with an investigation of the August meteors, discovered a great similarity in their orbit and that of Comet II. 1862. He therefore inferred their close physical association, and calculated the orbit of the November meteors with the idea of finding its parent comet. He, however, adopted an erroneous position for the radiant-point, and the resulting elements would not match with those of any known comet. In the 'Astronomische

Nachrichten' for Jan. 29, 1866, Prof. C. F. W. Peters, of Altona, showed that the elements of the November meteor group as computed by M. Le Verrier were almost identical with those computed by Dr. Oppolzer for Tempel's Comet (I. 1866). Four days later Schiaparelli, recomputing the meteoric orbit from more accurate data, discovered the same agreement, the relative elements being:—

	November Meteors.	Tempel's Comet (I. 1866).
Perihelion Passage	1866, Nov. 10·092	1866, Jan. 11·160
Longitude of Perihelion.	56° 25′ 9″	60° 28′ 0″
„ Node	231 28 2	231 26 1
Inclination	17 44 5	17 18 1
Perihelion distance....	0·9873	0·9705
Excentricity	0·9046	0·9054
Semi-axis major	10·340	10·324
Periodic time	33·250 years.	33·176 years.
Direction of motion ..	Retrograde.	Retrograde.

The resemblance is most significant, and may be said to have demonstrated the practical identity of comets and meteors. In publishing these results, Schiaparelli asked, "Are the meteors to be considered as swarms of minute comets or as the products of the dissolution of large comets?" The author of this important discovery received the gold medal of the R.A.S. in 1872, as a fitting tribute to his successful investigations.

The three successive returns of the meteoric group in about 1799, 1833, and 1866 offered some distinctions. In the former case the display was apparently confined to one night and to one year only. This would appear to indicate that the group existed in much more condensed form in 1799 than at the subsequent returns, for it was seen during the nine consecutive years from 1831 to 1839, and during the six consecutive years from 1864 to 1869. But it would perhaps be unsafe to argue that it did not appear in 1798, 1800, and 1801, as it might escape record, though it could hardly elude observation somewhere or other. Still it is certainly curious that no descriptions of it in the years immediately contiguous to 1799 have been found amongst the historical records of the period. And it is perhaps still more strange that in about the years 1733 and 1766 our chronicles are silent as to the apparition of a meteor-shower in the month of November. That it occurred in the years mentioned is highly probable. If the group formed, at the periods referred to, a far more compact one than at present it might only exhibit itself in a single year, instead of during a series of years as at the last two returns. It might also have passed the Earth during daytime in the most civilized parts of the world, and this would increase the chances of its escaping record. There are some other impediments which would assist in obliterating it, such as moonlight and cloudy weather; but, even allowing for them all, it seems unlikely that brilliant showers of Leonids, if they really occurred, could entirely escape mention. It is true that Dr. Dick, in his 'Sidereal Heavens,' p. 325, says, in speaking of the display of meteors of 1799, "They were seen by all the inhabitants of Cumana, the oldest of whom asserted that the great earthquakes of 1766 were preceded by similar phenomena"; but this appears to be little more than a tradition.

Comparing the showers of 1799, 1833, and 1866 it may be said that the one in 1833 was unquestionably the richest, and that the display of 1866 was the least imposing of the three. In 1833 it was estimated there were 1000 meteors per minute, while in 1866 there were about 100 per minute visible to one observer at the time of maximum display at $13^h\ 10^m$.

On discussing the times of occurrence of the various showers near the maximum epoch 1866 it became evident that some irregularities affected them, and that brilliant displays had taken place not always when the Earth occupied precisely the same longitude. A very abundant shower was witnessed by Mr. Bradford, at a place 50 miles N.N.W. from Pekin in China, on the morning of November 15, 1867, or some 12 hours after the maximum of the shower as seen in the same year at various places in the United States. Mr. B. V. Marsh compared the various times of occurrence, and from these it appeared that "the central and densest part of the meteor-stream through which the Earth passed in 1866 and 1867 is flanked on each side by lateral and somewhat less dense but wider meteoric currents at a distance from the orbit of the main stream, which the Earth crossed in about 12 hours on the mornings of November 13th, 1865, and November 15th, 1867" ('British Association Report,' 1869, p. 302). The meteor group, therefore, instead of consisting of a single mass evenly distributed on each side of the nucleus, would seem to be composed of a number of concentric streams, capable of originating a shower, fluctuating in intensity, and lasting altogether 48 hours.

W. F. Denning.

[To be continued.]

The Longitude of the Sydney Observatory.

From several points of view the question regarding the difference of longitude between Greenwich and Sydney is one of great importance, and any contribution which is likely to improve our knowledge respecting it may be thought acceptable. For some years the only method employed for determining the longitude of Sydney was that practised at our observatory, and known as the moon-culmination method. Professor Newcomb, of Washington, in his well-known work 'Investigations of Corrections to Hansen's Tables of the Moon' (Washington, 1876), says that the determinations of longitude from moon-culminations are found by experience to be subject to constant errors, which it is difficult to determine and allow for, and that it was therefore a part of the policy of the American Commission for the observation of the transit of Venus to depend on occultations rather than on moon-culminations for the determination of longitudes. The only direct determinations of the longitude of Sydney from the motions of the Moon are the following:—The Rev. W. Scott, the first astronomer in charge of the Sydney Observatory, gave $10^h\ 4^m\ 46^s{\cdot}66$ E. and $10^h\ 4^m\ 44^s{\cdot}43$ E. as values depending on meridian transits of the Moon observed at Greenwich and Sydney in 1860 and 1861, and at the Cape of Good Hope and Sydney in 1859 respectively (see Sydney Astronomical Observations for 1861, p. 82). Mr. Stone from a selection of the Greenwich and Sydney transits for 1859 and 1860, obtained $10^h\ 4^m\ 47^s{\cdot}32$ E. (see R. A. S. Monthly Notices, vol. xxvii. p. 302). Mr. Russell from a reduction in 1878 of the Greenwich and Sydney transits of 1863, 1871, 1872, 1873, and 1874 got $10^h\ 4^m\ 50^s{\cdot}81$ E. (see Introduction to the Sydney Observations for 1877 and 1878, p. 16). So long ago as 1861, I foresaw the great value which occultations of stars would have for the determination of the longitude of Sydney, and accordingly calculated a large number of occultations for observation at the Observatory. Many of these occultations were observed, but in consequence of their non-publication they have never been utilized. In 1864 I began observations of these phenomena at my own observatory which had then just been erected, and these observations have been systematically carried on down to the present time. In 1878, in a paper contributed to the local Royal Society on a proposed correction of the longitude of the Sydney Observatory, I advocated the method of lunar occultations. In this paper I deduced the longitude of my own observatory from ten occultation-phases of well-known stars, the Moon's places being corrected from transits at Greenwich. The result obtained was $10^h\ 3^m\ 22^s{\cdot}54$ E.

In a paper communicated to the Royal Astronomical Society in March 1880, I discussed twenty occultation-phases, the Moon's places being corrected from transit observations at Greenwich, the Radcliffe Observatory, and Washington, and found $10^h\ 3^m\ 21^s{\cdot}85$ E. These, I believe, were the first attempts in Australia to determine the longitude from occultations of stars. In 1884 Dr. Auwers investigated the longitude of Windsor from seventy-five occultations which I observed during the period 1873–76, comparing them with some hundreds of similar phenomena observed contemporaneously in the chief observatories of the northern hemisphere. He obtained $10^h\ 3^m\ 21^s{\cdot}25$ E. as the result. So important was this result regarded that Professor Harkness, of Washington, suggested that an accurate determination should be made by the telegraphic method of the difference of longitude of the Sydney and Windsor and the Melbourne and Windsor observatories. By the kind co-operation of the Acting Government Astronomer, Mr. H. A. Lenehan, at Sydney, and Mr. Ellery at Melbourne, this suggestion was carried out with very satisfactory results, the differences Sydney—Windsor, and Windsor—Melbourne being $1^{m}\ 29^{s}{\cdot}39$ and $23^{m}\ 25^{s}{\cdot}87$ respectively, the sum of these values, $24^{m}\ 55^{s}{\cdot}26$, representing of course the difference Sydney—Melbourne. This agrees very closely with the value $24^{m}\ 55^{s}{\cdot}40$ obtained directly between Sydney and Melbourne in 1883 (see the official report published in 1886). The longitude of the Melbourne Observatory, as deduced by Dr. Auwers in 1884 from twelve occultations observed at Melbourne in 1874–75, is $9^{h}\ 39^{m}\ 53^{s}{\cdot}12$ E. If, now, to the values obtained directly by Dr. Auwers for Windsor and Melbourne we add the differences $1^{m}\ 29^{s}{\cdot}39$, $24^{m}\ 55^{s}{\cdot}40$ respectively we get the longitudes $10^{h}\ 4^{m}\ 50^{s}{\cdot}64$ E., $10^{h}\ 4^{m}\ 48^{s}{\cdot}52$ E., which differ by $2^{s}{\cdot}12$. Dr. Auwers, however, gives the relative weights of the results from Windsor and Melbourne occultations as 5 to 1. I have just recently had the gratification of receiving from Dr. Hugo Clemens, of the Astrophysical Observatory, Potsdam, a copy of his inaugural dissertation entitled "Bestimmung der Länge von Windsor, N. S. Wales (Tebbutt's Sternwarte) aus Sternbedeckungen, 1864–70." It contains a very elaborate discussion of the longitude of Windsor from a series of 66 occultations observed by me in 1864–70, which are compared with some hundreds of similar phenomena observed during the same period in Europe and the United States. The resulting longitude is $10^{h}\ 3^{m}\ 20^{s}{\cdot}60$ E., and if to this we add the telegraphic difference before given, we get $10^{h}\ 4^{m}\ 49^{s}{\cdot}99$ E. as the longitude of the Sydney Observatory. The value for Sydney derived from purely telegraphic determinations is $10^{h}\ 4^{m}\ 49^{s}{\cdot}54$ E. (see the official Report of the Telegraphic Determination of Australian Longitudes published in 1886). This determination, however, if it stood alone would, for well-known reasons, be unworthy of great confidence. Finally, it is pretty certain that the value $10^{h}\ 4^{m}\ 48^{s}{\cdot}9$ E., given in the English and American 'Nautical Almanacs' for the current year, is too small. I am quite unaware on what grounds a reduction has been made in the value which obtained up to 1896.

Private Observatory,
Windsor, N. S. Wales,
1897, January.

JOHN TEBBUTT.

[With reference to the concluding sentence of Mr. Tebbutt's article, Dr. Downing kindly informs us that the longitude $10^{h}\ 4^{m}\ 48^{s}{\cdot}9$, given in the 'Nautical Almanac' for 1896 and 1897, is the value adopted by Mr. Stone in his Transit of Venus 1882 reductions (p. 55), and was given as the official value on the authority of Mr. Russell. In the 'Nautical Almanac' for 1898 and subsequent years the longitude of Sydney is given as $10^{h}\ 4^{m}\ 49^{s}{\cdot}54$.—EDS.]

The Rotation Periods of Venus and Mercury.

It is remarkable that of all the planets of the solar system, Mars and the Earth are the only two whose rates of revolution about their axes are known with any certainty. The obvious cause of this ignorance in the case of Neptune is its distance. Uranus has until recently not been observed for this purpose; and though rotation periods have been deduced for Saturn and Jupiter from observations of spots on their apparent surface, there are circumstances not inconsistent with the hypothesis that these markings are not rigidly attached to the planets, and hence that the periods are not those of the bodies themselves. Values of the period of axial rotation of Venus and Mercury were determined many years ago, and these have been accepted with a reliance, perhaps, not warranted by the data on which they depend. But within the last twenty years observations have thrown considerable doubt on these values, and it is for the purpose of recording some recent observations of this class that this note is written.

In our number for March of last year we gave a recapitulation of the observations of Venus for determination of axial rotation made to that date; since then we believe the only addition to these has been a series of drawings made by Mr. Percival Lowell and his assistant Mr. Drew, which go to support Prof. Schiaparelli's conclusion that Venus rotates in the same time as that in which she revolves in her orbit, and this is also held by other observers.

As to Mercury, it may be well to recall the circumstances of earlier determinations. The first is that by Schroeter, who in the years 1800 and 1801 observed the southern horn of Mercury's crescent to be slightly rounded, which he inferred was due to the interception of sunlight by a Mercurian mountain. By noting the recurrence of this phenomenon he deduced a rotation period of $24^h\ 4^m$. This was reduced by Bessel, who rediscussed the same observations, to $24^h\ 53^s$, with an inclination of the axis of the planet to its orbit of 70°, which result appears to have been accepted for many years. In 1884 Mr. Denning cast some doubt on its accuracy in consequence of some observations of a spot which he made on several mornings in November 1882, from which he inferred a period of about 25 hours*. In 1882 Prof. Schiaparelli, attacking the problem on a new plan, made his observations of Venus and Mercury, which are now classic. He observed the markings on the planets continuously from hour to hour, and was not able to detect any apparent change in the position of the markings which would be consistent with a period of about a terrestrial day, and from this he concluded that in the case of both planets the rotation is uniform with a period equal to that of the sidereal revolution.

The recent observations mentioned above are by Herr Leo

* 'Observatory,' vol. vi. p. 40.

Brenner, of Lussinpiccolo, and Mr. Percival Lowell. In Ast. Nach. no. 3387 (1896, Nov. 22), Herr Brenner published a series of drawings of Mercury made between 1896 May 19 and August 8, which show markings shadowy, but, judging by their shape, obviously the same, although drawn at times separated by intervals sometimes of hours sometimes of days, and from these he deduced a period of between 33 and 35 hours.

Mr. Lowell communicated his drawings made in September of last year to the Royal Astronomical Society in January last, and these he says are incompatible with any period of rotation, differing much from that of its orbital revolution. Mr. Lowell's drawings differ from Herr Brenner's in that the markings are shown as sharp lines; there are points so sharp and well defined that micrometric measurement might be made of one or more of these and a period deduced by accurate calculation. It is also worthy of notice that there is a marking continually seen near to the terminator which is also apparent in some of Herr Brenner's drawings, and which may be the same as one referred to by Mr. W. F. Gale, of New South Wales, who, writing to the Journal of the Brit. Ast. Assoc. in October last, said that he had examined Mercury with uncertain results, but, " beyond an ill-defined shading near the terminator, no markings were seen." It need scarcely be added that Schiaparelli's hypothesis requires that the terminator (neglecting the effect of libration, which is large for Mercury) should always be on the same part of the planet, and hence that a marking always seen near the terminator is evidence for the longer period.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Comet Tewfik.

Gentlemen,—

This comet was seen near the outer part of the solar corona during the total eclipse of the 17th of May, 1882. Has it been seen before or since? May I venture to reply, in all probability, both. A comparison of the photographs obtained in South America and in West Africa on the 16th of April, 1893, led to a suspicion of an appearance due to the presence of a comet in the interior part of the corona; and its changed aspect during the interval leaves little doubt that this was really the case. Admitting the comets seen in 1882 and 1893 to have been identical, and in or very near perihelion when seen, its period would be about 10·9 years. But an appearance of a similar kind was noticed by Prof. Winnecke (and a drawing of it published in the 'Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg,' vii^{e} série, tome iv. no. 1) on the occasion of the eclipse of July 18, 1860 (the first ever photographed). Now this was almost exactly 21·8 years, or two periods of 10·9 years, before May 17, 1882. To say the least, this is surely very remarkable.

For a reference to Prof. Winnecke's observation I am indebted to Ranyard's paper in the 'Monthly Notices of the R.A.S.' for June 1874 (vol. xxxiv. p. 369), where he calls attention to what seems to be an appearance of a similar kind on the photographs of the eclipse of December 12, 1871. He also refers (there and in 'M. N.' for November 1873, vol. xxxiv. p. 26) to a dusky spot with a bright nucleus, noticed by Pastorff on the Sun's disc on the 29th of June, 1819, which is believed to have been the great comet of that year (perihelion passage, June 28, early in the morning) seen projected upon the bright background of the photosphere; also to a very similar nebulous spot, with a small bright centre, depicted by Pastorff upon a drawing of the Sun's disc for the 26th of May, 1828, which may also have been a comet, though there is no record of one having been observed in any part of that year.

These other cases, however, have no reference to my object in this letter, which is merely to call special attention to three cases (on July 18, 1860, May 17, 1882, and April 16, 1893) when comets would seem to have manifested their presence in or near the Sun's corona during total eclipses, the times of which are consistent with the theory that all are appearances of one comet with a period of 10·9 years. That each of these was noticed on the occasion of a maximum of solar spots would seem, *pro tanto*, to give some probability to a suggestion which I ventured to bring forward in the 'Observatory' for August 1893 and October 1894 (vol. xvi. p. 294, and vol. xvii. p. 334), that the periodicity of the solar spots is due to a swarm of meteors revolving round the Sun, and approaching him very nearly when in perihelion, whilst receding somewhat beyond the orbit of Saturn when in aphelion. A comet or other body revolving in 10·9 years would have a mean distance of 4·92, somewhat less than that of Jupiter. It must be remembered that the *exact* period of the solar spots is not even yet known, while there have been lately several instances of protraction of the maximum, which may be due to the greater dispersion of the meteoric particles along the orbit, as is probably taking place in the Leonid meteors, and has been almost completely established in the Perseids. Yours faithfully,

Blackheath, 1897, March 3. W. T. Lynn.

The April Meteors.

Gentlemen,—

On the morning of 1803 April 20 a very fine meteoric shower was observed at Richmond, Va., and Portsmouth, New Haven. A similar phenomenon was witnessed on 1850 April 20, when an extraordinary display of meteors occurred at various places in India. In 1849, on the morning of April 20, three observers at New Haven counted 54 meteors in an hour, so that they were much more plentiful than usual. The interval between the showers of 1803 and 1850 is 47 years, and a like interval, since 1850, brings us to 1897. On the basis of these facts, I would therefore suggest that the sky be specially watched on the nights of April 19 and 20 next, to determine whether the Lyrids return in exceptional strength. The 47-year period I have pointed out rests on slender evidence, and may not be supported by further observation, but it is well that such a test should be applied. Unfortunately the Moon will be full on the morning of April 17, and her light will partly interfere at the important time, for she rises on the 19th at $11^h\ 7^m$, and on the 20th at $12^h\ 18^m$. A sharp watch should be maintained before these times, and it may be desirable to look out occasionally after moonrise, for in the event of a rich display the gibbous Moon would certainly not be able to obliterate it, though it would detract from its visible effects.

Yours faithfully,

Bristol, 1897 March 25. W. F. Denning.

Ball's supposed Discovery of the Duplicity of Saturn's Ring.

Gentlemen,—

My sole object in my letter on this subject in your February number was to correct the previous statement about the supposed omission of Ball's drawing in Sir J. Banks's copy of the 'Philosophical Transactions.' But as my friend Capt. Noble has again alluded to Kitchiner, who (as he had pointed out) was the first writer to refer to Ball's observation and draw the inference that he and his brother had noticed the division in Saturn's ring ten years before Cassini, may I just note that it is scarcely likely that Breen, Smyth, and Hind "blindly copied" Kitchiner, though they undoubtedly "followed" him in one sense of the word, *i. e.* drew the same inference that he had done from the paragraph in the 'Phil. Trans.,' in copies from which the plate containing the drawing had been omitted or (like the one first consulted by me) greatly misplaced. This appears certainly to have been the case with Breen, who quotes the actual words of suggestion in the 'Phil. Trans.,' which he attributes, not unnaturally, to Wallis, though we know now that they were by Moray, who says that he had written to Huygens on the subject.

More difficult is it to account for Proctor's definite statement ('Saturn and its System,' p. 49), that "William Ball discovered a black stripe of considerable breadth, running quite round the northern surface of the ring, and having its outer and inner edges concentric with the edges of the ring." This was certainly not taken from the mere suggestion of duplicity in the 'Philosophical Transactions.'

Yours faithfully,

Blackheath, 1897, Mar. 16. W. T. Lynn.

P.S.—Let me take this opportunity of correcting a misprint in my letter in your March number. At p. 135, line 21, insert the word "end" between "the" and "of."—W. T. L.

A Desideratum in Star Maps.

GENTLEMEN,—

Many a useful star atlas for the amateur has been published within the last half-century. That by Messrs. Ginn, of New York, described in the 'Observatory' for this month, is probably very good as far as it goes. But it has always seemed to me that, for the ordinary observer, the desideratum, holding a middle place between these small atlases and Argelander's, is a republication of the larger maps of the Society for Diffusion of Useful Knowledge, published in 1840. Being 2 ft. 2 in. square, these were suitable maps for hanging up against the Observatory walls. They contain stars to the 7th magnitude inclusive, and some of the 8th and 9th, the size saving the labour of turning over a number of small maps. Will no one undertake the reproduction of these? The figures in faint outline should undoubtedly be retained, both for fixing the stars on the memory, and for the sake of the curious astronomical gossip, so to speak, implied in them. The smaller maps of the S. D. U. K., 1830, were republished in 1883, improved as to the number of objects, but not in so clear type.

Melplash Vicarage, Bridport,
1897, March 10.

Faithfully yours,
S. J. JOHNSON.

NOTES.

COMET NOTES.—Messrs. Perrine and Aitken give new elliptical elements of Comet *g* 1896 (Perrine, Dec. 8) in Ast. Journ. no. 396. These are computed from 21 observations extending from Dec. 8 to Jan. 25, and agree very closely with those by Ristenpart given last month. They make the period 6·376 years, his value being 6·441.

The following ephemeris is for Greenwich midnight:—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Apr. 1	7 17 44	5 11	April 13	7 39 59	5 25
5	7 25 13	5 17	17	7 47 16	5 25
9	7 32 38	5 22	21	7 54 29	5 24

The brightness on April 9 is 0·008 of that at discovery.

J. G. Galle has re-determined the elements of Comet 1835 I. in Ast. Nach. 3409.

The new elements are as follows:—

$$T = 1835,\ \text{Mar. } 27^{d}\!\cdot\!24238 \text{ Berlin M.T.}$$

$$\left.\begin{array}{ll} \omega \ldots\ldots & 210^{\circ}\ 26'\ 22'' \\ \Omega \ldots\ldots & 58\ \ 20\ \ 32 \\ i \ldots\ldots & 170\ \ 52\ \ 32 \end{array}\right\} 1835\cdot0$$

$$\log q \ldots\ \ 0\cdot309663$$

This comet was discovered by Boguslawski on April 20, 1835, and was under observation till May 27. It was at all times faint and difficult to observe. The probable errors of the elements are therefore rather large.

A. C. D. C.

A GRAPHICAL METHOD OF COMPUTING THE CORRECTIONS TO BE APPLIED TO TRANSIT OBSERVATIONS.—The principle of the device which Mr. Seabroke introduced at the British Astronomical Association Meeting (see p. 160 of this number) we believe he does not claim as new; but it may be of sufficient novelty to many to warrant us giving a shortened explanation of it here.

If x'', y'', and z'' be respectively the level, azimuth, and collimation errors, the sum of the corrections to be applied to the transit of a star of dec. δ and zenith-distance θ will be $\frac{1}{15}$ sec δ $(x \cos \theta + y \sin \theta + z)$ seconds of time. Now take x and y as Cartesian coordinates, and lay down the point P they represent. Through the origin O draw a line making the angle θ with the axis of x; then if ON be drawn from P perpendicular to this line, ON will represent the quantity $x \cos \theta + y \sin \theta$, and, assuming the lines to be divided as uniform scales, the unit being a second of arc, it is easy to produce ON to M so as to add the collimation error, and it only remains to multiply the sum by sec δ and to divide by 15.

This is easily done by drawing the line OQ so that QOM = δ, and dividing the line OQ into a scale whose unit is fifteen times that of the other lines. From M draw MQ at right angles to OM, and OQ is the quantity required. It will be noted that QOX $= \delta + \theta$ = the latitude, so that the direction of OQ and the point P are constant for a night's work, and the line OM is variable, which leads to Mr. Seabroke's device. He draws the axes of coordinates and the direction OQ on paper, and plots down P. On a glass plate he draws a circle with a diameter corresponding to the direction OM and with a series of lines perpendicular to this. By superposing the glass plate on the paper with its centre at the origin and the ruled diameter in the position corresponding to the zenith-distance, the series of parallel lines on the glass make it easy to trace out the required point Q, and hence to read off the scale the required correction.

THE IRON LINES SEEN IN THE SPECTRUM OF THE HOTTEST STARS.—Prof. Lockyer has communicated to the Royal Society a preliminary note on some work he has done recently in continuation of his researches, published in the 'Proceedings' of the Society in 1879 and 1881. The earlier work consisted in noting (1) the lines brightened in passing a spark in a flame charged with metallic vapours, and (2) the lines brightened on passing from the arc to the spark. It was found in the case of iron that two lines in the visible spectrum at 4924·1 and 5018·6 on Rowland's scale were greatly enhanced in brightness and were very important in solar phenomena. Now, he has used a more powerful current and larger jar surface than formerly, with result that seven additional lines have been found to have their brightness enhanced at the highest temperature. The wave-lengths of these lines lie between 4233·3 and 4584·0; and Prof. Lockyer says, "Combining this with former results we seem justified in concluding that in a space heated to the temperature of the hottest spark and shielded from a lower temperature, these lines would constitute the spectrum of iron. Defining the hottest stars as those in which the ultra-violet spectrum is most extended, it is known that absorption is indicated by few lines only. In these stars iron is practically represented by the enhanced lines alone; those which build up, for the most part, the arc spectrum are almost or entirely absent."

A diagram is given showing that these enhanced lines are seen as bright, but of different intensity, in the spectrum of three hot stars—α Cygni, η Leonis, and Rigel, and also that three bright lines which are most marked at lower temperatures are almost entirely absent from the spectra of these three stars; but that in the spectrum of a relatively cool star, α Orionis, the seven enhanced lines are not seen, but that the group of three lines characteristic of low temperature is well marked; and the note goes on to say: "The facts illustrated by the diagram indicate that the enhanced lines may be absent from the spectrum of a star either on account of too low or too high a temperature. In the case of low temperature, however, iron is represented among the lines in the spectrum, but at the highest temperature all visible indications of its presence seem to have vanished.

"This result affords a valuable confirmation of my view that the arc spectrum of the metallic elements is produced by molecules of different complexities; and it also indicates that the temperature of the hottest stars is sufficient to produce simplifications beyond those which have so far been produced in our laboratories."

OBSERVATIONS OF JUPITER'S SATELLITE V.—In the last-published number of the 'Astronomical Journal' will be found some observations of this object, made by Prof. Schaeberle with the Lick 36-inch telescope on Feb. 27. These are very welcome, as the satellite has not been observed since 1894, and, as reference to our "Companion" will show, Mr. Marth was beginning to doubt the accuracy of his ephemeris owing to want of observations. Prof. Schaeberle says, "Marth's ephemeris is still in good agreement with the observed places, the error in the time of elongation being less than four minutes." He also says that although he had not previously attempted to observe the object, he found it quite readily by help of the ephemeris, the seeing being unusually good and the object near the meridian.

BINARY STAR ORBITS.—Continuing his crusade against determined orbits of binary stars which he considers untrustworthy, Prof. Burnham, in 'Popular Astronomy' for February, brings forward an especially bad case. The ellipse adopted for λ Cygni (OΣ 413) gives a period of 93·4 years. Prof. Burnham plots down the observations at twelve epochs, and remarks, "It is obvious from the distribution of these positions, as shown in the diagram, that it is impossible at this time to tell anything definite about the orbit of this pair, if it is a binary at all. In fact, a right line will represent the observed places best." The angular motion in 53 years has been 61°. We must therefore erase still another from our list of known binaries.

In the March number he gives the history of ξ Aquarii. It was first observed by Herschel in 1779, then by Struve in 1825, and from then measures have been fairly continuous to the present time. These observations are plotted down, and with them the ellipse which represents the orbit computed; but Prof. Burnham also draws an ellipse of quite a different kind which will equally well represent the observations. He concludes that it will require the measures of at least 500 years to furnish data for a definite solution of the problem.

MEETING OF THE ASTRONOMISCHE GESELLSCHAFT.—A report of the sixteenth meeting of this international astronomical society, held at Bamberg under the presidency of Dr. Weiss, in September last, is published in 'Ciel et Terre' for March 16. After the report of the state of the zone catalogue, which is being made under the auspices of the Society, had been read by the Secretary, Prof. Seeliger, the following communications were read:—"On the Relation between Luminescence and Astrophysical Phenomena," by Prof. Weidemann; "On Periodic Comets," by Kreutz; "On the Construction of Astronomical and Photographic Objectives." Dr. Hoffler made a contribution on the influence of the displacement of the solar system on the time that a ray of light takes to travel from Jupiter to the Earth. Other papers were read, and Budapest was selected as the place for the next meeting. Dr. Seeliger was elected as President. The journal from which we make this extract remarks on the agreeable social features of the reunion, which were largely due to Dr. Hartwig, the Director of the Bamberg Observatory.

OBITUARY.—Dr. WILHELM DÖLLEN. We regret to have to announce the death of this well-known Russian astronomer, which occurred on February 16 last. Herr Döllen was born in Mitau on 1820, April 25, and after studying at the University of Dorpat under Bartels and W. Struve, was in 1839 appointed Assistant in the Dorpat Observatory, where he did a large amount of meridian work. During this period he helped Dr. W. Struve, of whom Döllen was son-in-law, in the chronometric determination of the difference of longitude between Greenwich and Altona. In 1844 he was transferred to Poulkova, which observatory enjoyed the benefit of his untiring energy and devotion to research until his death. In 1890 when Otto Struve gave up the Directorship of the Poulkova Observatory, Dr. Döllen followed his example, and retired from official life to end the rest of his days at Dorpat, where he had spent his youth.

Dr. Döllen especially devoted himself to astronomy in the direction where it touches geodesy. He published annually an ephemeris of stars for time and azimuth determinations by observations with portable transit-instruments. It will also be remembered that on the occasions of total lunar eclipses since 1884 he has prepared and circulated among astronomers lists of the small stars to be occulted, which have been of considerable value in helping to find a value of the Moon's angular diameter.

The death is also announced of M. Antoine d'Abbadie, a distinguished geographer and astronomer, who was born in Dublin in 1818. He had travelled in Brazil and lived for several years in Abyssinia, and had published works on both these countries. He was elected a member of the Paris Academy of Sciences, and was a member of the expedition sent by that body in 1882 to St. Domingo to observe the eclipse of the Sun. He had also observed the Transit of Venus of 1874.

Though he was not an astronomer, we may record here the death of Prof. James Joseph Sylvester, Savilian Professor of Geometry in Oxford University. Prof. Sylvester, who was second wrangler in 1837, was successively professor of natural philosophy in University College, London, of mathematics in the University of Virginia, and then at the Royal Military Academy, Woolwich. Later, he returned to America as the first professor of mathematics in the newly founded Johns Hopkins University. During this period he edited the American 'Journal of Mathematics,' and it is said that he practically founded the study of higher mathematics in the United States. In 1883 he was elected to the Oxford Chair in succession to Henry Smith. He died on March 15 in his 83rd year.

The Story of the Weather.—We have had sent us a little book with this title. The publishers are Messrs. George Newnes and Co., and the author is Mr. G. F. Chambers. Taking these things in conjunction with the title, it does not require much effort to imagine the kind of book it is. Mr. Chambers has compiled a handy little book on meteorology, which consists of descriptions of instruments used in the science, and a collection of remarks of the nature of weather predictions. There is much useful information, though it is doubtful whether the predictions would be realized in many cases; but the author guards himself by stating in his preface that the book is not meant to be exact meteorology, so we must not quarrel with him. We do, however, object to his statement on p. 13 when he speaks of the immense number of observations (presumably meteorological) which lie buried in the 'Greenwich Observations' from 1750 to the present time, which have not been utilized except by Mr. Glaisher. Mr. Chambers does not seem to know that the meteorological observatory was not established until 1837, and he is certainly not doing justice to other meteorologists.

THE ARGENTAURUM PAPERS.—We are occasionally favoured by having sent to us publications of the kind which some hard-hearted people at once consign to the waste-paper basket. This is not our way, but we think it right to sometimes mention these if only to show that the race of persons to whom Prof. de Morgan devoted a whole volume is not yet extinct. We have lately had sent us a largish book by a certain gentleman whose name we will not give, but will describe him as he describes himself. He is the author of at least thirty works on various subjects, ranging from Home Rule to the Constitution of Nickeliferous Pyrrhotite; he has patented scores of inventions, he has made diamonds, he has been the largest maker of arsenic in the world, and he has established a society for the abolition of compulsory poverty. All this evidence of the versatility of his genius he brings forward to convince certain scientific men (he names Sir Robert Ball, Lord Kelvin, M. Flammarion, and Prof. Tait) that he is a fit and proper person with whom they might argue without loss of dignity on the subject of the rotation of the Moon on its own axis, which our author thinks it does not; and also on certain points in which he thinks Newton was wrong, such as the elementary proposition that a sphere attracts as though its mass were concentrated at its centre. There are some original ideas on geometry in the book, which, in case anyone would like a little amusement in reading, we may say is sent from the Argentaurum Laboratory, 20 Central Avenue, New Brighton, New York.

MESSRS. STANFORD have kindly sent us copies of new editions of Mr. Lynn's books 'Celestial Motions,' 'Remarkable Comets,' and 'Remarkable Eclipses,' the latter naturally containing a short account of the circumstances of the phenomenon of last August. We can only reiterate what we have before said as to the utility and cheapness of these little books.

MR. PERCIVAL LOWELL asks us to announce that on and after April 1st the address of his observatory will be "Flagstaff, Arizona."

WE call attention to the occultation of Mars by the Moon on the morning of April 9; but its observation will scarcely be possible in the neighbourhood of Greenwich, owing to the nearness of the planet and Moon to the horizon.

FROM AN OXFORD NOTE-BOOK.

A CURIOUS instance of wilfulness or waywardness on the part of a translator was brought to light the other day by the query of a correspondent. He asked on what grounds the suggestion of the term "*Apex* of the Solar Motion" was attributed to Herschel, and he was referred to Herschel's classical paper of 1783 March 6

(Phil. Trans. vol. lxxiii.), where at the top of p. 273 we read:— "With respect to the change of declination, I would observe that the point of λ Herculis which in fig. 4 is assumed as the Apex* of the solar motion is not perhaps the best selected."

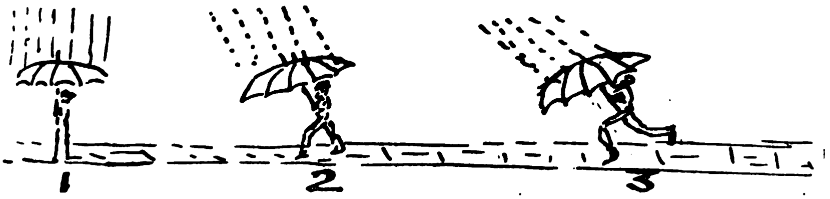
The asterisk refers to a footnote:—"I use the term Apex here to denote that point of fig. 4 wherein all great circles, drawn through the supposed direction of the motion of the solar system, intersect, and which, in other stereographic projections, is generally a pole, either of the ecliptic or equator. As this point is in the northern or elevated hemisphere, the Sun, by tending to it, may be said to ascend, and the term Apex may perhaps not be an improper one."

My correspondent replied that he had known this paper "only from an abstract in Bode's 'Astronomisches Jahrbuch für 1787,' where the new term and the note concerning it are omitted." And on turning up the reference I was astonished to find that the omission seems to have been of the most deliberate character. The translation in Bode is not a mere abstract, but a full translation of the original paper, practically word for word. The sentence above quoted, however, is rendered as follows:—"Wegen der Abweichung muss ich bemerken, dass die Gegend um λ im Herkules, auf welcher die Sonne zuzugehen scheint, wol noch nicht die richtigste seyn mag." And the note is omitted altogether. One is at a loss to understand why the suggestion made so modestly, and which time has shown to have been a good one, should have been treated with such scant courtesy.

Dr. Nansen's book was announced in the 'Times' of February 15, and reviewed to the extent of three columns, and many of us will probably have read it by this time. I don't know whether the writer or the reviewer is responsible for the following simile:—"The ship should be of such a build as to offer the greatest resistance to ice-pressure. She should be made *as comfortable as a fixed observatory.*" This sentence set me thinking, but my reflections are not of much importance alongside the description given later in the review of the standard of comfort actually adopted. "Abundant and varied meals were served with regularity. A windmill was erected for the purpose of working the dynamo that gave the company a more or less regular supply of electric light. The ship was provided with an excellent library of books grave and gay. Games there were of sorts, an organ out of which to grind tunes, and musical instruments of other kinds." Now I wonder how much of this is the proper equipment of a fixed observatory? They have just got electric light at Greenwich, and there is some excellent grave (and a little gay) reading in the library. Also they play hockey with great success. But the musical instruments, now?

In an examination held the other day it was asked: "What are aberration and parallax?" The following answers—two about aberration and one about parallax—have merits of their own:—

Aberration is apparent alteration of direction owing to the rapid movement of the observer, thus:



The same shower and the drops falling vertically to an observer at rest as 1 will show more aberration as the rate of progress of the observer increases as in 2 and 3.

Aberration is the inclination of something to something else, caused by the motion of one; *e.g.* aberration of light is caused by the Earth's revolution round the Sun.

Parallax is the *apparent* meeting of two parallel lines; *e.g.* stellar parallax, by which the stars appear in the same position of the heavens as viewed from the Earth in different points of its orbit.

To these I may perhaps add the following, which I found put away at the bottom of the drawer where such scraps are allowed to accumulate. The examination this time was not in astronomy, but in Shakspeare's 'Coriolanus,' and comments were invited on the phrase "Bemock the modest Moon." One of the comments was as follows:—"Spoken by Menenius to Coriolanus when he tells him that he has seen his own face in the Moon." Another was ventured by one bearing the illustrious name of Newton:—"It means that Coriolanus would deride even the Moon, who hurt no one."

THE last paragraph mentions the bottom of the drawer; and, indeed, it had got so full that it was necessary to clear it out. Unfortunately much of the material found therein is not of great value—it had better have gone into the waste-basket at once. But at the present time of year, when Annual Reports keep one busy, material of even a modest kind must not be despised: and I take the opportunity of reprinting a few extracts from popular works which illustrate the use made of astronomical imagery in fiction. The first item is purely descriptive and is due to William Black, 'In Silk Attire' (p. 37):—

There was no Moon as yet, but there was clear starlight, and as they drove past the Green Park the long rows of ruddy lamps hung the far darkness like strings of golden points, the counterpart of the gleaming silver points above. And there away in the north glimmered the pale jewels of Cassiopeia; the white star on Andromeda's forehead stood out from the dark sea; Orion coldly burned in the south, and the red eye of Aldebaran throbbed in the strange twilight. The dark grey streets and the orange lamps and the tall houses and the solitary figures of men and women hurried past and disappeared; but the great blue vault, with its twinkling eyes, accompanied the carriage-windows, rolled onward with them, and always glimmered in.

BUT authors are seldom content with description pure and simple. The author of 'Uncle Remus,' in a story called 'Free Joe,' introduces the following sentences :—

> Helen did not follow this timely advice at once. From her window she could see the constellations dragging their glittering procession westward: and she knew the spirit of the night was whispering gently in the tall pines, but her thoughts were in a whirl.

Mr. Thomas Hardy makes a more deliberate and detailed use of the stars in 'A Pair of Blue Eyes' (p. 180):—

> Their conversation detained them on the lawn and in the portico till the stars blinked out. Elfride flung back her head and said idly:
>
> "There's a bright star exactly over me."
>
> "Each bright star is overhead somewhere."
>
> "Is it? Oh, yes! of course. Where is that one?" and she pointed with her finger.
>
> "That is poised like a white hawk over one of the Cape Verde Islands."
>
> "And that?"
>
> "Looking down upon the source of the Nile."
>
> "And that lonely quiet-looking one?"
>
> "He watches the North Pole, and has no less than the whole equator for his horizon. And that idle one low down upon the ground, that we have almost rolled away from, is in India, over the head of a young friend of mine, who very possibly looks at the star in our zenith, as it hangs low upon his horizon, and thinks of it as marking where his true love dwells."

AND perhaps I may wind up with a few quotations from the pages of 'Happy Thoughts.' I wonder how many of our readers remember the Astronomer in 'Happy Thoughts,' and whether any of them know that he was a real person—the late J. R. Hind, to wit? "I had it from one, that had it from him." Here is his picture by Mr. Burnand ('Punch,' 1866):—

> Being in need of quiet, in order to commence my great work on Typical Developments, I have found a charming retreat on the banks of the Thames, somewhere about Twickenham, or Teddington, or Richmond, or Kingston, and all that part. . . . .
>
> I discover for the first time that I've got a neighbour who looks at the Moon and Jupiter every night through a large telescope. He asks me would I like to step in and see Jupiter? . . . . . . I have stepped in and seen Jupiter (who gave me some difficulty in getting himself into focus) until my head aches. No writing to-night.
>
> (Next night.) Neighbour over the hedge asks me "Would I like to have a look at Jupiter?" I say I won't trouble him. He says no trouble, just get the focus, and there you are. He *does* get the focus, and consequently there I am. I leave my 'Typical Developments,' Chap. 1. . . . . . Looking through the telescope makes one's head ache. We *did* have some brandy-and-water. Shan't stop up so late again. Cocks begin to crow here at midnight. It's quite light at midnight. . . . .
>
> 4th and 5th. Happy days. Disturbed in the evening by Jupiter, Saturn, and the Moon, which have always got something remarkable the matter with them.

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, April 9, 1897.

Sir R. S. Ball, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., and
H. F. Newall, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Newall. There is a long list of presents made to the Society since the last Meeting; the number of these is seventy-one. No doubt each of them will be found of interest by some one; but there is one to which perhaps I might call special attention, and that is the 'Annals of the Astro-physical Observatory at Meudon,' published by M. Janssen, the Director. This volume contains a number of prints of photographs of sun-spots which have attracted much attention.

A vote of thanks was passed to the donors of the presents.

The President read the names of several Fellows who were suspended for non-payment of subscription.

Prof. Turner. We have received an interesting paper from *Prof. S. W. Burnham* on the Orbit of Sirius. Prof. Burnham states that previous to 1896 the companion to Sirius was last measured by himself in 1890 with the 36-inch refractor at the Lick Observatory, when he secured satisfactory measures on several nights. A great many orbits had been computed, agreeing more or less, but these depended on observations confined to a comparatively limited portion of the orbit. These orbits had periods ranging from 49½ to 58½ years, and Prof. Burnham suggests that in finding these shorter periods the computers may have been influenced by variations of proper motion deduced from meridian observations. Using his observation of 1890 Prof. Burnham deduced an orbit with a period of 51·97 years, which he published in 'Monthly Notices,' vol. liii. p. 482, and also in the Publications of the Lick Observatory. This present paper relates to a recent observation by Prof. Schaeberle, which is useful for checking the accuracy of orbits determined from previous observations, and with this he finds an orbit with a period of 51·80 years, which is very little different from his previous determination.

Mr. Seabroke. Does he give the distance of the companion at the present time?

Prof. Turner. Yes. 3·71 seconds of arc.

The President called on *Mr. A. W. Roberts*, of Lovedale, South Africa, to read his paper on "Personality in Observation of Variable Stars."

Mr. Roberts. Many observers of variable stars have noticed the peculiarity that when two stars are seen in the field of a telescope, one immediately under the other, the lower of the two stars appears relatively brighter than if it were above the other star. Of those who have commented on this illusion I may mention the name of Safarik; of Prof. Pickering, who, in the Harvard Annals, vol. xiv., mapped out a curve of the error due to this cause, which influences variable star observations. Also Prof. Barnard, when observing, not long ago, the two satellites of Uranus, noticed that when one was above the other, the lower was apparently the brighter, and when the position was reversed that still the lower appeared the brighter. This was mentioned in the 'Astronomical Journal,' No. 370, and Prof. Barnard said that he thought this was due to a physiological effect, and that his eye had a tendency to make the lower of two equal lights brighter by about ½ magnitude. Dr. Chandler, when observing the variable U Pegasi, noticed that when he had the star below the comparison star it always appeared brighter than when it was above. Four years ago I had some correspondence with Dr. Gould, who advised me to follow up this question, and I have endeavoured to find the source of this error, which is vitiating all observations of stellar brightness. To do this I selected a group of six stars, and observed these at all possible hour-angles, so as to see them at different position-angles relative to one another, always using first an ordinary astronomical eyepiece, and then a terrestrial eyepiece, which reversed their position. The differences in brightness as observed in these two positions are represented in these diagrams as curves. The abscissæ represent the angles that the line joining the star under consideration to the *centre of the field* makes with the vertical, the ordinates represent the differences, and it will be seen that each star apparently has a light curve dependent on its position-angle, and that each appears brightest when the position-angle of the line joining it to the centre of the field makes an angle of 32° with the vertical. I have little doubt, more especially as a similar thing has been noticed by Prof. Pickering, Dr. Chandler, and Prof. Barnard, but that this represents a subjective reality. The results of such observations would all be influenced by this source of error, and that they are so influenced is shown by the large number of suspected variables which find their way into catalogues, nine out of ten of which have to be set aside on examination and most of which are said to have a period of about one day. Another thing which strengthens my argument is that in most of the elements of variable stars there will be found a secondary term which, I believe, is due to this illusion. It may occur to some persons that this depends on the telescope used. My instrument is a common theodolite, and I have turned it every possible way, and have used very many different eyepieces, to make sure that this is not an instrumental effect; and I may add that I have reduced my observations by the method of least squares, so that the curves that you see here are the actual result of an analytical process and are not in any way affected by bias in drawing.

Dr. Downing. What is the range of magnitude of this error? So far as I follow Mr. Roberts, the size of the error depends upon the position-angle of the variable with respect to the centre of the field. Can he give us a formula connecting the error with this position-angle?

Mr. W. Bryant. I should like to ask if the angle of 32° was found to be an absolute constant or a personal constant depending upon the observer?

Prof. Turner. If a professional astronomer were asked to give an example of real and unselfish astronomical work, he could not select a much better instance than the present one—the devotion of several years to the examination of one small point, small but nevertheless vitally affecting the accuracy of observations. I am sure we all appreciate the reference to our late Associate, Dr. Gould, as the mainspring of this inquiry.

Capt. W. Noble. There must be some physiological effect at the bottom of this, having its origin in the eye, akin to that which causes us to see the constellations so monstrously larger when near the horizon than they appear to be in the neighbourhood of the zenith—a phenomenon of which no satisfactory explanation has appeared in any book on astronomy that I have ever seen. That this is so, seems to me to be shown by this peculiar change of magnitude incident on the reversal of position of two stars, alike striking Professor Barnard in California and Mr. Roberts in South Africa, using instruments, moreover, differing so widely in power and construction as they did.

Mr. Seabroke. It is curious that the error increases the farther stars are away from the centre of the field. Also it appears from these diagrams that the retina increases in delicacy from the lower part upwards.

The President. Capt. Noble's remark as to the large size of the constellations applies equally to the appearance of the Sun and Moon, which seem to have an increased disk as they approach the horizon.

Capt. Noble. I have seen a balloon come over my head when it must have been within half-a-mile of me, and it looked a little mite of a thing, and I have watched it descending two or three miles off, when it has seemed comparatively enormous as it came near to the earth.

Mr. Thwaites. May I ask whether Mr. Roberts has noticed any astigmatism in his eyes?

The President. I think the number of questions which have been asked shows the interest taken in Mr. Roberts' paper. We must thank him for having brought the matter before us, and we are glad to think that we have a Fellow of this Society working so diligently as he is doing in a far-off land.

Mr. Roberts. As to the formula giving the magnitude of this error, I cannot find any relation involving the distance apart of the stars, except the general one that the further the star is from the centre of the field, the greater is the variation. As regards position-angle, the error is represented by the formula $R \cos(\theta - 32^\circ)$. In this first star I found a difference of a whole magnitude between the greatest and least observed magnitudes. In answer to the gentleman who asked if I were troubled by astigmatism, I may say that this subjective phenomenon that I have been investigating is also experienced by Prof. Barnard and others, and it is hardly likely that all our eyes are at fault. It was also asked if the value of the position-angle 32°, which gives the maximum effect, is absolute. I can only say that it is absolute for me—that is, it is the same for all groups of stars that I have examined.

Mr. Knobel read a paper on "Some Original Unpublished Observations of the Comet of 1652." He said that in a copy of Bayer's 'Uranometria' he had recently obtained at Cologne he had found recorded a series of observations of the position of the comet from Dec. 19, 1652, to Jan. 3, 1653, made by alignment with the stars. This comet had been carefully observed by Hevelius. The observations under notice recorded the position of the comet on three days not included by Hevelius. On comparing these observations with those made at Danzig, they appeared to be as good as could be expected from the method adopted. One of the observations seemed remarkable inasmuch as it apparently indicated the position of the comet on January 20, 1653, about ten days after Hevelius had declared the comet was no longer visible.

Photographs of these observations were thrown on the screen, and Mr. Knobel called attention by the way to the fact that the unknown author had designated the group of stars in Aries made into the constellation "Musca" by Bartsch, as "Nubes Tauri, vel Halitus Arietis," the origin of which he explained, but added that he was quite unacquainted with those names for this constellation.

The President. Mr. Knobel's interesting communication is of a kind which scarcely calls for criticism or discussion; but we all thank him for unearthing and giving to the Society this old record.

Mr. A. R. Hinks read a paper on "Personal Equation depending on Magnitude affecting the R.A. of the Stars in the Cambridge Zone Catalogue of the Astronomische Gesellschaft Catalogue, and its determination from Astrographic Catalogue Plates." This paper had its origin in some work that had been done recently by direction of the Director of the Cambridge Observatory. The Astronomische Gesellschaft Zone Catalogue which has been made at Cambridge overlaps that made by Dr. Becker at Berlin, and there are on this account 671 stars common to the two catalogues. On comparing the places as given in the two catalogues, it was found that the Right Ascensions determined at Cambridge were systematically larger than those determined at Berlin by about $0^{\mathrm{s}}\cdot07$ in the mean. There was reason for thinking that this might be due to a variable personal equation, and consequently these differences were grouped according to the magnitude of the stars; the variation of magnitude of the difference with the magnitude of the star was then apparent. It happens that the zone of the Astrographic Catalogue which is being photographed at Oxford is the same as that which is being observed at Cambridge; and Prof. Turner kindly furnished some data deduced from measures of astrographic plates. The fundamental stars used for reduction of the Oxford plates were those observed at Cambridge; and when the residuals, that is the differences between the Cambridge places and those of the photographic plate, were computed, it was seen that they agreed pretty fairly with the differences Cambridge – Berlin, which pointed to the fact that the personality was largely in the Cambridge observations. This point was of interest, as it showed that with very little trouble the plates of the astrographic chart might be used to determine the uncorrected personal equation in the Astronomische Gesellschaft Catalogue.

Captain Noble. Anyone who has observed as many thousand transits as I have will know how much more accurately that of a small star can be observed than of a large one; for while the division of the interval between the two places of a minute point of light, by the wire, at two successive ticks of the clock, admits of accurate estimation [illustrating on the blackboard], the disc of a bright star like Capella or α Cygni often overlaps the wire at the instant of a tick in a way which renders its exact time of passage very difficult indeed to determine.

Dr. Downing. At the meeting of the International Astronomical Conference held in Paris last year, it was discussed whether any correction for magnitude shall be applied to the places of the stars which are to form the International Fundamental Catalogue; and it was decided not to introduce a correction of this kind owing to the uncertainty of its amount; but recommendations were made that directors of observatories should investigate this matter and make whatever determinations they could; and it is a matter of satisfaction to me that the Cambridge staff have not been slow to adopt the recommendation of the Conference.

Prof. Turner. Some time since I came to a determination to see what could be done to eliminate or determine personality by a photographic method, as mentioned in the last report of the Oxford University Observatory to the Society. Mr. Hinks wrote to me saying that he was making a similar research, but on independent lines, and I was glad to put such material as I had at his disposal. The number of plates finally reduced at Oxford was not very great, but I hope in a short time that we shall have a good deal more material, and Mr. Hinks has expressed his willingness to undertake the discussion of these when they are ready in the same way as he has done in the paper which he now brings before the Society.

Mr. Newall. It may be of interest to call attention to one point in connection with this personal equation depending upon magnitude which may afford a hint to those who are devoting their attention to this subject, and that is what may be called the apparent pendular movement of two stars, which may be described thus:—If there are two stars in the field of a telescope, a large one and a small one, one above the other, and a slight shake be given to the instrument, the small star will seem to swing with respect to the brighter star, the faint star lagging behind the bright one. I think we ought to congratulate Mr. Hinks on the result of his investigation, which must also be gratifying to Mr. Graham, who made most of these observations. It seems a fortunate circumstance that Oxford and Cambridge should be working at the same zone—the one from a photographic, the other from a visual point of view.

Dr. Common. I think that this pendular appearance depends a great deal upon which star you are looking at while you shake the telescope.

Mr. Stone. I think this variation of personal equation with magnitude in eye and ear observations depends a great deal on the method of observation. Some persons observe by dividing the distance between the positions of the star at two consecutive seconds, others by mentally dividing the time interval between two clock-ticks. One of the observers at the Radcliffe Observatory adopts a combination of these two methods with some success.

The President in thanking Mr. Hinks for bringing forward this subject mentioned that this catalogue which Mr. Graham has been occupied on for a number of years is now finished, and will be distributed shortly.

The Astronomer Royal. I thought it might interest Fellows if they saw the first-fruits of work with the new Thompson photographic equatorial obtained by Mr. Dyson and Mr. Cowell last night. [Photographs of stars taken with the 26-inch refractor inside and outside the focus and at the focus were shown on the screen.] I have also brought some photographs taken by Mr.

Lewis with the 28-inch refractor adjusted for photographic work. [Photographs taken with the 28-inch inside and outside the focus, and also photographs of Castor, γ Leonis, ξ Ursæ Majoris, ζ Boötis, and the Moon were shown on the screen.] We have got the new instrument into fair working order now, the distance between the lenses having been carefully adjusted after a series of trials, and the images appear to be very sharp, and, so far as one can judge, it promises very good results for double stars. Means have been devised in the object-glass for adjusting the distances between the lenses. These photographs show that the object-glass is very nearly in proper photographic adjustment. There is one thing I would call the attention of our Fellows to as regards this application of photography. I think that these photographs with the 28-in., which may be looked upon as experimental in the first instance, show there is a great field for photography in this direction in measuring double stars. With the 28-inch telescope the diameter of the disc of the fainter component of Castor is only about one-third of a second of arc on the photograph, and we have distinctly measurable photographs of a double star, the components of which are only half a second of arc apart. I have brought these forward to-night to give the Society an idea of what we hope to get in the future.

Dr. Common (who now occupied the Chair in the absence of the President). I have had the pleasure of looking at some of these photographs, and I may say I was very much struck with the extraordinary beauty of the images, especially with the images of the double star. Their delicacy is surprising, and I think the Astronomer Royal is right in supposing that photography may really supersede the eye even in such a delicate operation as the measurement of double stars. I have no doubt that Mr. Burnham would contest this statement, but these first trials with the telescope are good, and no doubt with better nights and more experience and possibly better plates the Astronomer Royal will have some very excellent photographs to show us.

The Astronomer Royal. Several of those photographs were taken with Dr. Hill Norris's dry collodion plates, and I think the character of the images has been better in consequence.

Dr. Common. I am pleased to hear that you have tried those plates. Some of the results got with dry collodion plates are astonishing. In photographs of nebulæ or of the Moon, definition is of the first importance, and last year I made experiments with the ordinary dry plates and with dry collodion plates. I took as a test-object a newspaper hung up against the wall, and using an ordinary quarter-plate camera with a ½-inch symmetrical lens exposed a dry plate. I examined the picture I got, and found that with the lightning-plate the letters ran into one another, so that they could not be made out, but with a slow plate one could make out the letters, and with the dry collodion plate their form could be seen distinctly. I also found that on a photograph taken with a dry collodion plate I could distinguish the 300 divisions round a watch of ordinary size.

Prof. Turner read a paper by *Mr. A. F. Lindemann* on an improved method of clock-winding, and said that Mr. Lindemann's method of winding had been in constant use in his Observatory for the last eight months with great success.

Prof. Turner then read a short paper on "The Straightness of of Spider Lines." He had heard that the straightness of the horizontal wire in the transit-circle at an observatory which he preferred not to mention had been suspected of curvature or "sag." If such "sag" existed generally it would enter directly into the R − D observations, and would to some extent affect all Z.D. observations. To determine whether it existed in the horizontal wire of the Greenwich transit-circle, he had examined all those cases where a star had been observed at vertical wires 1, 2, 6, and 7, and subtracted the readings for 1 + 7 from those for 2 + 6, correcting the result for curvature. The general result obtained from some hundreds of such observations was that the wire was sensibly straight. As regards the suspicions in the case already mentioned, he believed they were based on observations of polar stars, and it was possible that the complete correction for curvature had not been applied. Near the pole terms of higher orders than the second were required, which could be expressed very simply in rectangular coordinates.

The following papers were announced and partly read:—

R. T. A. Innes. "A new Quadruple Stellar System."

H. H. Turner. "On the Straightness of Spider Lines."

John Tebbutt. "Observations of the Minor Planet ⑧ Flora."

S. W. Burnham. "Orbit of Sirius."

T. J. J. See. "Micrometrical Measures of the Double Stars in the Great Nebula and Cluster surrounding η Carinæ, made with the 24-inch Refractor of the Lowell Observatory."

E. B. Knobel. "On some Original Observations of the Comet of 1652."

E. J. Stone. "On the Determination of the Epoch-correction of an adopted System of Right Ascensions of Clock-stars from observations of the Sun, including the Effects of Personalities; and applications of the results to the determination of the Errors of the Tables of the Sun and Moon."

A. W. Roberts. "An Investigation concerning the Position-error affecting Eye-estimates of Star Magnitudes."

A. Marth. "On the Apparent Disc and on the Shadow of an Ellipsoid."

Radcliffe Observatory, Oxford. "Observations of Comets 1896 III. (Swift) and 1896 IV. (Sperra)."

A. F. Lindemann. "An Improvement in Winding Equatorial Clocks."

A. R. Hinks. "Preliminary Note on a Personal Equation depending on magnitude affecting the R.A. of the Stars in the Cambridge Zone Catalogue of the Astronomische Gesellschaft, and its determination from Astrographic Catalogue Plates."

The following gentlemen were elected Fellows of the Society:—

John Franklin-Adams, Wimbledon.

Rev. Sherrard Beaumont Burnaby, M.A., The Vicarage, Hampstead, N.W.

John Sisson Slater, M.A., LL.D., Seafield House, Lytham, and 1 Garden Court, Temple, E.C.

Rev. John Spence, 27 Walpole Street, Chelsea, S.W.

The following Candidate was proposed for election as a Fellow of the Society:—

Prof. Henry C. Lord, Director of the Emerson McMillin Observatory, Ohio State University, Columbus, U.S.A. (proposed by George E. Hale).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fifth ordinary meeting of the current session of the British Astronomical Association was held at University College, Gower Street, on Wednesday, March 31, the President, *Mr. N. E. Green, F.R.A.S.*, in the Chair.

Four new members were elected, and the names of seventeen candidates for Membership were read and suspended. These included thirteen in connection with a proposed new Branch in Melbourne.

Mr. Maunder announced that the Royal Botanical Society had offered the Association a site within their grounds for the erection of an astronomical observatory, and that the Council had that afternoon decided to accept the offer. The Council considered that probably nowhere within London could so good a site be found for an observatory as Regent's Park. A Committee had been formed to carry out the negotiations, who would report to the next Meeting.

The President announced that the next Meeting of the Association would be held in Essex Hall, Essex Street, Strand, in order to give the Members an opportunity of forming an opinion as to the relative convenience of the neighbourhood of the Strand as compared with their present meeting-place.

Mr. J. Evershed read a paper explanatory of a spectroscope for solar prominence observation which he had recently designed, his object being to meet the requirements of amateurs and others without having recourse to complicated and expensive apparatus. In this spectroscope the necessary dispersion is secured by the employment of two compound prisms made of the densest silicate Jena glass supplied by Hilger. These give a very high dispersion with excellent definition. A novel feature in the design is the use of a plane mirror placed behind the first prism, which reflects the light back at an angle of about 35° into the second prism and observing telescope. The mirror is movable by a screw in the plane of dispersion, the centre of motion being so placed that the various coloured rays emerging from the first prism are reflected into the second prism centrally and at their appropriate angles of incidence. A position can be found for this axis which satisfies the conditions exactly. The prisms, therefore, are fixed, and no motion is required in the observing telescope, which is fixed permanently in a convenient position for observation. Thus the whole length of the spectrum can be rapidly surveyed by merely turning the milled head actuating the mirror. The framework of the spectroscope consists of three parallel and equidistant hollow steel rods, connected together by three rings of cast aluminium. The rods pass through holes in the castings, the latter being held firmly in position by set screws. The collimator and telescope are screwed into brass plates attached to the central ring, and the prisms and mirror are arranged in a circular disc of hard wood attached to two of the rods, between the second and third rings. The brass plate to which the small telescope is screwed projects from the central ring at an angle of about 35°, and is stayed with sheet brass to the prism table, so as to secure perfect freedom from flexure in all positions. When the instrument is attached to an ordinary refracting telescope for solar observations, the ends of the three steel rods fit into long holes in a brass collar, which fits on the rack-tube, and are firmly clamped in position by thumbscrews. The principal advantages of the design are: (1) compactness and strength combined with minimum weight, the instrument weighing only 3 lbs.; (2) simplicity of the adjustments, the range of the instrument being limited to the visible spectrum, no focussing is required for the collimator; (3) rapidity with which the whole spectrum can be surveyed; (4) freedom from distortion —the prisms being direct-vision, the spectrum lines are undistorted images of the slit.

Mr. Maunder observed that Mr. Evershed's instrument was a marvellously compact and handy one.

Four papers dealing with Jupiter were then read. "Jupiter in Perfect Definition," by *Mr. Henry MacEwen*; "The Present Aspect of Jupiter," by the *Rev. W. R. Waugh*; "The Recording of Eye-estimate Transits of prominent Jovian Spots," by *Mr. MacEwen*; and "The North Equatorial Belt of Jupiter," by the *Rev. W. R. Waugh.*

The President read his Report of the Saturn Section for 1896, and *Mr. Petrie* read an Interim Report of the Mercury and Venus Section by the Director, *Mr. Henry MacEwen.* The Director especially alluded to a series of drawings of Venus by the *Rev. T.*

H. Foulkes, made in Malta, which he considered one of the finest series of drawings ever made of Venus.

Mr. Maunder read a paper by *Herr Brenner* criticising Mr. Lowell's recent papers on the Rotation Period of Venus. Herr Brenner compared two drawings of Venus, one by Herr W. Villiger, of May 31, 1896, and the other by M. Stuyvaert, of March 4, 1892, from which he deduced a rotation-period of $23^{h}\ 57^{m}\ 36^{s}\cdot37728$, which agreed very exactly with the mean of his 14 most reliable drawings, viz. $23^{h}\ 57^{m}\ 36^{s}\cdot2396$, and argued that a mere accidental agreement was highly improbable. Considering these facts it was incomprehensible how there could still be partisans of the rotation period of 224·7 days. The writer proceeded to say that Venus had never before been observed under such unfavourable circumstances as by Mr. Lowell. An inexperienced observer, an unsuitable telescope, an unhappily chosen eyepiece, a very small diameter of the planet, observed with an insufficient power, a low declination, all together explained Mr. Lowell's peculiar drawings, which resembled rather a collection of Chinese coins than our nearest planetary neighbour.

Capt. Noble said that Mr. MacEwen had not referred in his report to the rotation period, but the observation made by Mr. Foulkes of a well-defined hump like a lunar mountain, which disappeared in forty minutes, seemed inconsistent with the long rotation theory.

Mr. Griffiths said that he could always see most unmistakable markings on Venus whenever he observed her. He thought Herr Brenner's remarks on Mr. Lowell's observations were not quite fair.

Mr. Crommelin remarked that there was a difficulty in reconciling the long period of rotation with the fact that Venus was probably a young planet, comparable at any rate with the Earth as to age. Assuming that the two planets began life with a similar rotation period, it was almost inconceivable that the solar tides should have been capable of lengthening it to 225 days.

Mr. Holmes said that a good deal of the paper would be well worth reading again, substituting "Herr Brenner" where they had read "Lowell," and "Mars" where they read "Venus."

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, the 21st April, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. E. Mawley, F.R.H.S.*, President, in the Chair.

Mr. W. H. Dines read a paper "On the Relation between Cold Periods and Anti-cyclonic Conditions of Weather in England during the Winter." There seems to be a generally accepted belief that anticyclonic conditions during the winter are likely to be accompanied by exceptional cold; but in so far as England is concerned the author's observation has led him to the opposite conclusion; and he always expects a frost to break up as soon as the barometer gets much above 30·00 inches. To test the truth of this theory he tabulated the height of the barometer during all the cold periods during the three winter months of the fifty years 1841–90. Out of 74 frosts, he found that 16 only had a pressure exceeding 30·20 inches, and the majority of these were of very short duration. Thirty-three, or less than half, had a pressure exceeding 30·00 inches. Twenty-one had a pressure below 29·80 inches, and these included almost every frost in the period remarkable for its length or severity.

A paper by *Mr. A. Lawrence Rotch*, of the Blue Hill Observatory, Mass., was read describing the use of kites at that observatory to obtain meteorological records in the upper air. Three kinds of kites have been used, viz.: 1, the Malay kite, which presents a convex surface to the wind; 2, the Hargrave cellular kite; and 3, a flat kite with a fin or keel on the front devised by Mr. Clayton. These kites are attached to a wire carrying self-recording meteorological instruments, and a steam-winch automatically distributes the wire on the drum and records its pull. The instruments have been elevated more than one hundred times, and valuable meteorological data as to the changes of temperature, humidity, and wind up to an extreme altitude of 8740 feet above Blue Hill have been obtained.

A paper by *Mr. A. B. Macdowall* on "Suggestions of Sun-spot influence on the Weather of Western Europe" was also read. The author believes that there is a tendency to greater heat in the summer half-year, and to greater cold in the winter half-year near the phases of minimum sun-spots than near the phases of maximum; the contrast between the cold and heat of the year thus tending to be intensified about the time of minimum sun-spots.

The Great Meteoric Shower of November.

[Continued from p. 169.]

THE radiant-point of the meteoric shower of 1866, from a mean of six observations by Adams, Baxendell, Brunnow, Challis, J. F. Herschel, and A. S. Herschel, was at 149° 12′ +23° 1′, while the position of the cometary radiant lies at $150^{\circ}\cdot5$ $+23^{\circ}\cdot5$ according to the computations of Prof. A. S. Herschel. The agreement in place is very satisfactory considering the character of the observations upon which the former radiant is based. The theoretical meteor-speed is 44 miles per second, and this approximately accords with the observed velocity, but the latter feature is extremely difficult to determine with any approach to accuracy. These meteors belong, in fact, to the swiftest class of these bodies, as they are moving from that point towards which the Earth is travelling in her orbit. This is about 90 degrees preceding the Sun along the ecliptic, and is known (since Prof. Pritchard first employed the expression) as the apex of the Earth's way. The observed or apparent speed of the meteors is therefore a combination of the orbital motion of the meteors (about 26 miles) and of the Earth (about 18 miles).

When important discoveries are announced, or any remarkable conclusions drawn from existing observations, it is generally the case that they meet with serious opposition if not virulent criticism. Prof. Schiaparelli's views on the close association of comets and meteors were, however, strengthened by the able and concise researches of Newton and Adams, supported by many connected facts of unequivocal kind and by agreements in orbital elements which could not be regarded as fortuitous, so that they do not appear to have encountered much antagonism or unfavourable comment. The concluded relationship of comets and meteors was acknowledged to be a legitimate deduction from observed facts, hence the critic was disarmed and the ground cut away from under the feet of sceptics.

Schiaparelli's views were briefly enunciated in the 'Monthly Notices,' vol. xxxii. p. 194 *et seqq.* He says that "small bodies forming cosmical clouds probably occupy a large portion of the celestial spaces and that the motion of these dust clouds may be similar to that of the fixed stars. When attracted by the Sun they are not visible unless they receive an orbit which is an elongated conic section. Whatever may have been the original form of the cloud it cannot penetrate far into our system without assuming the form of an elongated cylinder passing gradually into a stream of particles. The number of such streams seems to be very great. The particles are so scattered that their orbits may cross each other without interruption and may possibly be always changing like the beds of rivers. The stream, after passing its perihelion, will be more diffuse than before, and when passing a planet may be so violently affected as to separate or break up, and even some particles may assume quite a new orbit and become independent meteors." In explanation of diffuse radiation, Prof. Schiaparelli remarks that a cloud of meteors having been deflected from its originally parabolic orbit into one of moderate period by perturbation from a large planet, the foremost and swiftest particles of the swarm will gain on the slowest, with the ultimate result that the stream will be continuous throughout the ellipse. Should the two ends, before reuniting, suffer further disturbance from planetary action, the front end of the wreath will outlie the rear end, and the stream will assume the form of a spiral curve of a single coil. Succeeding revolutions will add further coils and the "wreath of meteoroids, without losing its continuity, will become a belt of many strands overlying and interlacing with each other." (See B. A. Report, 1871, p. 45.)

It is true that Schiaparelli was not the first to infer that an alliance existed between comets and meteors. Several writers had indeed pointed out that the group of meteors through which the Earth passed in 1799 and 1833 must have formed a body similar to that of a comet. In the 'Danville Review' of December 1861 Prof. Kirkwood, of Indiana, gave his opinions as follow:—

1. That meteors and meteoric rings are the *débris* of ancient but now disintegrated comets, whose materials have become distributed around their orbits.
2. That the separation of Biela's comet as it approached the Sun in December 1845 was but one in a series of similar processes, which would probably continue until the individual fragments would become invisible.
3. That certain luminous meteors have entered the solar system from the interstellar spaces.
4. That the orbits of some meteors and periodic comets have been transformed into ellipses by planetary perturbation.
5. That numerous facts, some observed in ancient and some in modern times, have been decidedly indicative of cometary disintegration.

In effect, this is a pre-conception of the theory modelled and elaborated by Schiaparelli a few years later, but there is a considerable distinction between the two researches. While Kirkwood broached it simply as a theory without facts and calculations adequate to support it, Schiaparelli gave it as a definite conclusion, based on reliable data and mathematical deductions which practically demonstrated it. We must remember, however, that at the time Kirkwood wrote, the principal facts utilized by Schiaparelli were unknown, for neither the comet of the August meteors (1862 III.) or the comet of the November meteors (1866 I.) had been seen nor had the bright meteor-showers of August and November in the years from 1863 to 1866 been observed. Thus Schiaparelli had the advantage not only of considering the views of previous labourers in this field, but could avail himself of new materials which were of the utmost significance. The opportunity was a remarkable one and Schiaparelli accepted it. Marshalling his facts in proper array he brought the skill of the mathematician and the reasoning of the logician to bear upon them successfully. He proved himself the man for the hour.

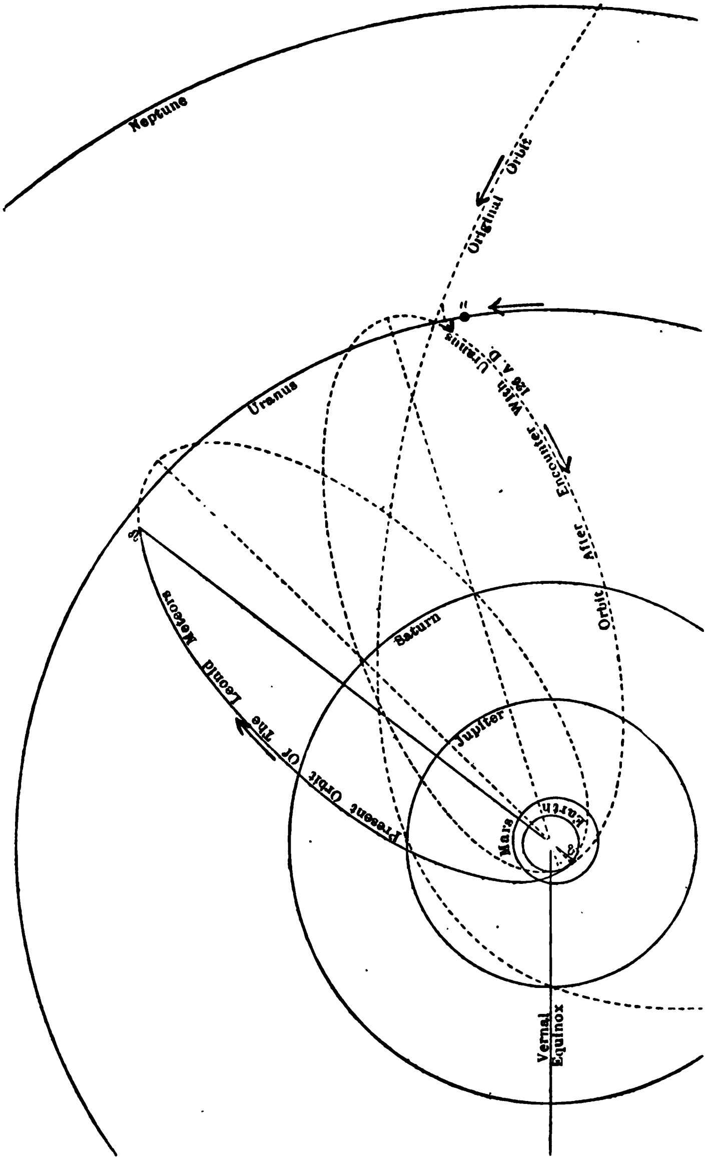
When Tempel's comet (1866 I.) was found to be the virtual progenitor of the November meteor-shower, a large amount of attention was directed towards the observations which had been secured, and the orbit received careful investigation. Dr. Oppolzer of Vienna, computed definitive elements, which showed that the perihelion was reached at a point just inside (2,200,000 miles) the Earth's orbit, while the aphelion was situated outside the orbit of

Uranus. The periodic time of revolution he gave as 33 years and 65 days. Prof. Newton suggested that the comet of 1366 was probably identical with it, and it has been supposed that the comet of 1133, observed in China, might represent a still earlier apparition. The ancient comets referred to were of considerable brilliancy, whereas Tempel's comet of 1866 was decidedly faint, so that were the three objects identical a great decrease of size must have been effected, probably by a process of dissolution involving a great waste of material.

The interval of five centuries between the comets of 1366 and 1866 represents a mean period of 33·28 years from 15 returns, which is 38 days in excess of that found by Oppolzer. Kirkwood points out that "with this value of the periodic time and the known secular variation of the node, it is found that the comet and Uranus were in close proximity about the beginning of the year 547 B.C. It is therefore not improbable that the former was then thrown into its present orbit by the attraction of the planet. The comet will make a near approach to the Earth about the 16th or 17th November, 1965, and to Uranus in 1983. At one of these epochs the cometary orbit will probably undergo considerable transformation." These views are rendered very doubtful by the great uncertainty attaching to the elements of the comet of 1366, which was only observed roughly on a few nights, so that its identity with that of 1866 is far from being proved. The annexed diagram (p. 200), from 'Popular Astronomy,' March 1897, represents the past and present orbits of the November meteor comet.

Le Verrier considered it probable that the comet was drawn into its present orbit by the action of Uranus, to which it made a near approach in the spring of the year 126 of the Christian era. Further observations of the comet and meteors during their ensuing return in about 1899 will enable this point to be reinvestigated on more satisfactory lines. The comet will probably pass its perihelion in April, and ought to be observed in February, though the conditions affecting the return are not so favourable as those which prevailed in the winter of 1865–66, when the Earth and comet were near together on the same side of the Sun. The comet, though undoubtedly a faint object, will scarcely be able to elude the diligent search that will be made for it with some of the large telescopes now existing. It may now, of course, have become really fainter and less compact than in 1866 by the further dispersion and exhaustion of its materials, for that comets do exhibit signs of progressive physical decadence seems very probable from the evidence afforded by Halley's comet, amongst others, the splendour of which in ancient times offers a strong contrast to its moderate brightness at modern returns.

Conspicuous displays of Leonids appear to have ceased after 1869, but the habitual observers of meteors, as they pursued their watches at the middle of November, never failed to recognize some indications of the shower's return. As regularly as the epoch


Neptune
Original Orbit
Uranus
Orbit After Encounter With Uranus A.D. 126
Saturn
Jupiter
Mars
Earth
Present Orbit of the Leonid Meteors
Vernal Equinox

came round, a few bright, swift, streak-leaving meteors appeared amongst the stars of Gemini, the Lynx, Ursa, Draco, and Virgo, and attested, by the directions of their flights, the activity of the well-known radiant in the sickle of Leo. To summarize all the observations since 1869 would afford no useful purpose, and it will be sufficient to quote a few of the results as specimens of the whole:—

	h m h m	
1870, Nov. 13	14 10 to 17 15	21 Leonids seen by Prof. A. S. Herschel at Glasgow.
14	12 0 to 17 33	53 meteors observed by 5 observers at Greenwich. 45 of these were mapped, and included 28 Leonids.
1871, Nov. 12	10 15 to 18 30	55 meteors, of which 20 were Leonids, observed by S. J. Perry at Stonyhurst College.
13	11 30 to 13 30 14 0 to 14 30	42 meteors, including 14 Leonids, seen at Harkhurst inKent.
14	16 0 to 18 0	32 meteors, 26 of them Leonids, seen by Prof. A. S. Herschel, Newcastle-on-Tyne.
1872, Nov. 13	12 0 to 18 0	638 meteors counted by several observers at Matera, Italy. The display much brighter than usual this year.
1875, Nov. 11 & 14.	......	32 meteors (13 Leonids) seen at Stonyhurst. 26 meteors (11 Leonids) seen in 4 hours on November 14 at Greenwich.
1877, Nov. 13	13 55 to 15 45	54 meteors, chiefly Leonids, seen by 2 observers at Bloomington, Indiana (Prof. D. Kirkwood).
1879, Nov. 13	5 30 to 17 0	100 meteors (18 Leonids) observed in a watch of 11½ hours by the writer at Bristol.
12, 13, 14	10 0 to 18 0	309 meteors (104 Leonids) seen by 2 observers on the three nights at Stonyhurst.
1880, Nov. 11 & 12.	......	12 Leonids recorded by Sawyer at Cambridge, Mass.
1885, Nov. 14, 15, 16, 17.	......	114 meteors (11 Leonids) seen in 10 hours by the writer at Bristol.
1887, Nov. 14	12 30 to 17 15	29 meteors (7 Leonids) seen in 3½ hours by the writer at Bristol.
1888, Nov. 13	15 30 to 18 0	29 meteors (17 Leonids) seen in 2½ hours by the writer at Bristol.
1890, Nov. 13 & 14	......	31 meteors (14 Leonids) seen in 3½ hours by Backhouse at Sunderland.
1895, Nov. 12, 13, 16, 17.	......	96 meteors (34 Leonids) seen in 9½ hours by Corder at Bridgwater.
13	11 30 to 15 40	34 meteors (16 Leonids) seen in 4½ hours by Blakeley at Dewsbury.
1896, Nov. 13	14 0 to 17 20	33 meteors (15 Leonids) seen in 3½ hours by Corder at Bridgwater.
14	14 0 to 18 15	66 meteors (43 Leonids) seen in 4 hours by Corder at Bridgwater.
14	12 0 to 18 0	46 meteors (34 Leonids) seen in 6 hours by Prof. A. S. Herschel at Slough.

In 1896 the shower exhibited a well-defined increased in numbers as compared with the figures for 1895. But the display was not a brilliant one, the horary number of Leonids being only 8 or

10, for one observer, at the richest time, viz. the morning of November 15. Summaries of observations in 1895 and 1896 will be found in the 'Monthly Notices,' vol. lvi. p. 255, and vol. lvii. p. 277.

It is evident from the circumstance that every November brings its Leonids, many or few, that this system of meteors forms a continuous ellipse, though the particles appear to be irregularly distributed and to be by no means numerous in certain parts. But even at that portion of the orbit opposite to the place of the parent comet, there are sufficient fragments to constitute a well-defined meteoric shower. In presence of the facts deduced from observation it is difficult to understand the statement recently made by Dr. G. J. Stoney, that "the meteors being not yet, as they undoubtedly would be in course of time, drawn out into a continuous band extending all round their orbit, was in favour of their recent origin—that was, recent from a cosmical point of view—and a very accurate determination of their orbit might enable the hypothesis to be verified" ('English Mechanic,' vol. lxv. p. 32). Though the meteors do extend all round the orbit they are not so evenly distributed as the Perseids, and probably had a more recent introduction into our system. But the Leonids may have formed a fine display many ages before the first historical record of them 1000 years ago. As early as the years 931 and 934 the stream was so distended that the richer portion apparently occupied 3 years in crossing the point of intersection with the Earth's orbit. Even then, as now, the stream may have been complete, for we have no evidence to the contrary. From the observed dispersion of the swarm in 931–4 we are warranted in assuming that it came permanently into the solar system at a comparatively remote period, and long before the meteoric group of Biela's comet. Some of the other leading showers are undoubtedly older than the Leonids, and among these may be instanced the Quadrantids, Lyrids *, Perseids, Orionids, and Geminids, for these show no great periodical revivals such as affect the Leonids. At any rate we have little evidence that they are grouped in enormous strength at any particular region of their orbits. They recur annually and in fair abundance, but do not furnish meteor-showers similar to that seen at the maximum epochs of the Leonids or Andromedes. It must be remembered, however, that meteoric phenomena have only been properly watched during a relatively short series of years, and that several of the showers now supposed to manifest themselves annually, without any great periodical condensation, may have maxima at very long intervals of time. Comets are known to have periods of revolution varying from 3½ to more than 100,000 years, and meteoric systems probably display the same diversity of orbit. This branch of astronomy having been entirely neglected

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* The statement here made must, perhaps, be qualified as regards the Lyrids, for they appear to have offered a brilliant display in April 1803.

before the present century, it is certain that we can learn little more from the records of the past than has been already gleaned, but must appeal to the patient and accurate observation of the future to clear up all doubtful points. W. F. Denning.

[To be continued.]

The Appearance of Halley's Comet in A.D. 141.

In the tenth volume of the 'Monthly Notices' the late Dr. Hind has traced the history of Halley's Comet, and, incidentally to this research, he compared the elements of its orbit with observations made by the Chinese. Among the earlier apparitions of the comet is mentioned the one that took place in A.D. 141. Dr. Hind took his data from the work of Pingré ('Comètographie,' tom. i. fol. 292); he showed that the elements of the comet of A.D. 1066 (which is Halley's Comet) would satisfy the observations recorded in the Chinese Annals, but, as was usual with him, he did not give an exact account of his fundamental data and methods of discussion, thereby leaving his, otherwise very valuable, investigations incomplete. My own researches in a similar field have confirmed the general accuracy of most of the results obtained by Hind; but my investigations have been pushed a step farther, giving the limits between which the elements of the various comets may vary; and when occupied with the record of the comet of A.D. 141, my discussion of the observations and the calculation of the elements has given probability to the supposition that Dr. Hind made his calculation, not from a judicious discussion of the observations, but that he in the first place assumed the apparition to have been really a return of Halley's Comet, deducing a set of elements which would satisfy the observations and which at the same time resemble closely those of Halley's Comet. Dr. Hind's probable method is the one generally pursued in tracing the history of the comet, but while in this instance it has given a correct result, it cannot be always recommended, it being certainly better to discuss the observations themselves and to deduce the probable elements from them. A case in point is the comet observed in the year 1668, the observations having been made by certain Jesuits of the Collegio Romano, while stationed in Goa. We have a calculation of the orbit by Henderson, and I have deduced some elements which satisfy in a more or less degree the observations, but do not satisfy me, and, last of all, it has been shown that the elements of the great comet of 1843 will completely satisfy these positions. Of course when only two observations are available it is the only course to pursue, unless, indeed, the laborious method of trial, by assuming different values for Olbers' M, be resorted to.

The comet observed by the Chinese in A. D. 141 is in fac Halley's Comet; and as at present some astronomers are occupying themselves with completely working up the history of this most celebrated comet, it may be of interest to them to have some indications in regard to one of its earlier apparitions. I here make public the statement of the early observations, a catalogue of the stars composing the Chinese stellar divisions in which the comet was seen, and the possible elements of the orbit as deduced from these observations. From time to time, when my investigations of Chinese observations will allow me to do so, I shall publish similar accounts with regard to the other apparitions of Halley's Comet.

The following is the record of the observations of this comet:—

"In the year 6th of the epoch *Young-ho*, the 2nd Moon, the day *Ting-çe*, a comet was seen in the east. Its length was 6 or 7 cubits and its colour was a bluish white. It pointed to the south-west to *Ying-ši*, extending to *Fan-mou*. The day *Ting-čao* the comet was about one degree in *Koui-siou*, its length was 6 cubits. The day *Kwei-wai* it was to the north-west. It passed through the *Maou-siou* and *Py-siou*. The day *Kya-šin* it entered the eastern part of *Tsing-siou*. It passed through *Lieou-siou*, *Čang-siou*, into the middle of *Hien-yuen*, and then disappeared."

The epoch *Young-ho* is the period A.D. 136–141, hence the 6th year is A.D. 141. For the times of observation we find:—

2nd Moon, day *Ting-Çe*:	March 27th.
day *Ting-Çao*:	April 16th.
day *Kwei-wai*:	April 22nd.
day *Kya-Šin*:	April 23rd.

As to the stellar groups and divisions, we have the following:—

Ying-ši is the *Ši-siou* and is defined by α and β Pegasi.

Fan-mou are the four stars γ, ζ, η, π Aquarii.

Koui-siou is formed by the stars η, ζ, ι, ε, δ, π, ν, μ, and β Andromedæ, σ, τ, λ, υ, φ, χ, ψ Piscium.

The *Maou-siou* are the Pleiades.

Py-siou is determined by α, γ, δ, θ, ε, λ, and σ in Taurus.

Tsing-siou is a very extensive domicile constituted by μ, ν, γ, ξ, ε, *d*, ζ, λ Geminorum.

The *Lieou-siou*, identical with the Hindu *Âçlešă*, is constituted by δ, ε, ζ, ω, θ, ρ, η, σ Hydræ.

The *Čang-siou* is determined by φ, λ, μ, κ, υ Hydræ. (Williams, in his 'Observations of Comets,' London, 1871, has copied an error committed by Biot, writing ν^1 Hydræ in place of υ Hydræ.)

Under the apellation *Hien-yuen* are comprehended the following stars: ρ, ο, α, η, ζ, μ, ε, κ, α, and 10 Leonis Minoris.

From these extremely rough data the elements had to be derived. The only passably satisfactory observations are the first three, of which the first is very uncertain; and then the most serious source of error is in the times of observation, which may be in error as much as one day either way. Hence the calculations had to be made with three different sets of solar co-ordinates, besides having to take into account possible errors in the geocentric positions of the comet. The work was very considerable and not very satisfactory in its results, but still grouping the resulting elements in such a way as to leave no doubt of its identity with Halley's Comet. While the identity can be considered established, I do not consider myself as free from the obligation to extend and perfect the discussion, a work which I have to defer to some other day, and which will be most profitably undertaken when all the appearances of Halley's Comet can be treated as a whole.

The following are the mean values of the co-ordinates of the first three observed positions, as referred to the equinox of A.D. 141·0:—

A.D. 141	March 27·5	Paris M.T.	$\alpha_I = 313^\circ \pm 6^\circ$	$\delta_I = -3^\circ \pm 8^\circ$
,,	April 16·5	,,	$\alpha_{II} = 347^\circ$	$\delta_{II} = +14^\circ$
,,	April 22·5	,,	$\alpha_{III} = 29^\circ$	$\delta_{III} = +18^\circ$

The orbital element deduced from these observations which remains most constant is the inclination, which is $i = 163^\circ$, *i. e.* the inclination to the elliptic of the orbit of Halley's Comet. For the longitude of the ascending node the extreme values may be between $☊ = 358^\circ$ and $☊ = 35^\circ$, not so very much different from the value it has in the real orbit. The value of the argument of perihelion has a decided variation and may be anywhere between $\omega = 80^\circ$ and $\omega = 170^\circ$, its mean being almost identical with Hind's value ($\omega = 120^\circ\ 55'$). The perihelion distance comes out between $q = 0{\cdot}55$ and $q = 0{\cdot}95$, and the time of the passage of the perihelion may be anywhere between March 23rd, A.D. 141 and April 6th of the same year. If the dates of observation were diminished by about from one third of a day to one day, the resemblance of the orbit to that of Halley's Comet would be much closer. If I may be permitted an opinion on this subject, it is to the effect that the Chinese observations, however rough, will permit a determination of the passage of perihelion sufficiently close to test the results of the calculation of the perturbations. Once determined, with all possible exactness, the practical value of the Chinese observations, the theory of the comet can be entirely left to the researches of the computing astronomer.

Milan, Italy. 1897, March 9. GUSTAVE RAVENÉ.

The Longevity of Astronomers.

THE longevity of astronomers has often been mentioned as a fact. The Herschels, the Cassinis, and others have certainly lived to great ages, notwithstanding the fact that their avocation necessitated irregular hours and exposure to night air at all seasons. Some time ago, in order to while away a leisure hour, I compiled a list of the names of well-known men connected with astronomy, who had lived beyond the allotted human span of "three score years and ten." This list was by no means complete (the authorities I consulted having been insufficient for me to make an exhaustive summary), but it was very lengthy; and as it may possibly have some little interest, I transcribe a portion of it here, without, however, claiming that it proves anything. I have omitted all instances where the age fell below 80 years. In some cases the year given is a little less or a little more than the exact age: thus J. H. Mädler at the time of his decease was 76 days short of being 80 years of age, while W. Olbers was 81 years and 5 months old. It will be observed that the names included are not always those of practical astronomers. A very large number might be selected of those who died between 75 and 80, such as Argelander, Bode, Boscovich, Challis, James Cassini, Derham, Euler, Galileo, Hevelius, John F. W. Herschel, La Place, La Lande, Le Grange, Maskelyne, Admiral Smyth, T. W. Webb, Lord Wrottesley, &c. There are very few men indeed who, like Jeremiah Horrocks (1641, æt. 22) and W. Gascoygne (1644, æt. 23), have died very young, but left honoured names in astronomical history.

The subject is an interesting one, and it might be useful to form a complete list of the life-durations of the most eminent men who have successfully associated themselves with astronomy. If the exact dates of birth and death were given, the list would supply a ready means of reference to those scientific writers who have occasionally felt the want of such information.

	Obiit.	*Æt.*
Fontenelle, Bernard de ...	1757	100
Herschel, Caroline L. ...	1848	98
Cassini, Count J. D.	1845	97
Sabine, Sir Edward	1883	94
Mairan, De	1771	93
Somerville, Mary	1872	92
Santini, Giovanni	1877	91
Sharpe, Abraham	1742	91
Long, Dr. Roger	1770	90
Airy, Sir George Biddell .	1892	90
Thales	B.C. 550	90
Humboldt, Alex. von ...	1859	90
Robinson, Rev. T. R. ...	1882	90
Bouillaud, Ismael	1694	89
Rosenberger, Prof. Otto A.	1890	89
Gautier, Jean Alfred	1881	88
Biot, J. B.	1862	88
Cassini, J. D.	1712	87
Messier, Charles	1817	87
Wallis, J.	1703	87
Brewster, Sir David	1868	86
Halley, Edmund	1742	86
Schwabe, Samuel Heinrich	1875	86
Barlow, Peter	1862	86
Pingre, Alex. Guy	1796	85
Longomontanus	1647	85
Horrebow, P.	1764	85
Whiston, William	1752	85
Pritchard, Rev. Charles ...	1893	85
Maclear, Sir Thomas ...	1879	85
Hutton, Dr. Charles	1823	85
Dick, Dr. Thomas	1857	84
Woolhouse, W. S. B. ...	1893	84
Newton, Sir Isaac	1727	84
Le Monnier, Peter Charles	1799	84
Herschel, Sir F. William	1822	84
Lee, Dr. John	1866	83
Bernouilli, Daniel	1782	82
Troughton, Edward	1835	82
Olbers, Dr. William	1840	82
South, Sir James	1867	82
Le Gendre, Jean	1833	82
Nasmyth, James	1890	82
Eratosthenes	B.C. 195	81
Aristarchus	? B.C. 280	81
Emerson	1882	81
Moestlin, Michael	1631	81
Maurolico	1575	81
Bernouilli, John	1748	81
Kant, Immanuel	1804	80
Lassell, William	1880	80
Piazzi, Joseph	1826	80
Mädler, J. H.	1874	80
De Lisle, Joseph N.	1768	80
Bacon, Roger	1294	80
De La Hire, P.	1718	80

Bristol, 1897.

W. F. DENNING.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Lunar Eclipse of January 28, 1888.

Gentlemen,—

In Canon Taylor's 'Leaves from an Egyptian Note-Book,' the last chapter gives some extracts from the diary and letters of one of the ladies of the party. Under date January 29th [1888] she says: "We sat up last night to see the eclipse of the Moon, and it was well worth our while. The appearance of the stars was most wonderful. They were hardly visible before the eclipse began, owing to the brilliant light of the Moon. 'It was *so* fortunate,' as a lady remarked to me, 'that we happened to have a full Moon for the eclipse.' The Moon darkened till nothing was left of her but a bronze disc, with a ruddy glow in the centre; then the stars streamed out in dazzling beauty, and as the light of the Moon reappeared, they gradually faded away again. The whole population of Luxor perambulated the village for two hours, chanting prayers and beating tom-toms. The next day we got the following explanation from one of the natives: 'Moon go wrong; want Lord to make Moon go straight. If Moon go wrong, we not know when sow corn, when say prayers.'"

The lady who thought it so fortunate that they were favoured with a *full* Moon at the time of the eclipse was not so observant as Thucydides (probably she was younger at the time than he was when he composed his *κτῆμα ἐs ἀεί*); for the great historian, speaking (ii. 28) of the solar eclipse which occurred in the first year of the Peloponnesian war, says that the Moon was then New, and that it appears that such a phenomenon can only take place when this is the case (*ὥσπερ καὶ μόνον δοκεῖ εἶναι γίγνεσθαι δυνατόν*).

Canon Taylor's party were, however, fortunate in having a fine night (Jan. 28) in which to witness an eclipse of long duration (the totality lasted $1^h 38^m$), and that in a locality specially favourable, for the Moon was nearly vertical in Upper Egypt during the first part of the eclipse. It will be seen that, in common with other observers farther to the north, they speak of having noticed the deep-red colour in the centre of the Moon's disc; and perhaps it may not be out of place to refer here again to Prof. Filopanti's speculations on its cause, to which you called attention in the number of the 'Observatory' for March 1888 (vol. xi. p. 157.) He strongly expresses the view that this colour arises not only from the solar light refracted in its passage through the Earth's atmosphere, but also from some physical or chemical change peculiar to the Moon's surface, which he conceives, after having been exposed for a long time to a very strong direct solar light, to keep up for a while (as has been noticed in other opaque bodies) a sort of phosphorescence. It is, however, difficult, on this view, to understand why this deep red light in the centre of the Moon's disc should not be seen on the occasion of every lunar eclipse. In some, for instance that of June 10, 1816, she has become wholly invisible.

Yours faithfully,

Blackheath, 1897, April 5. W. T. LYNN.

The Lunar Eclipses of 1898.

GENTLEMEN,—

No less than three eclipses of the Moon visible at Greenwich occur within the limits of next year, 1898. This is noteworthy, inasmuch as for more than two centuries and a half such a coincidence will not happen again. In 1917, indeed, there are three eclipses of the Moon, one of which, that of December 28, commences as the Moon sets, so there would be three visible in Ireland that year, but not at Greenwich. In past years this has occasionally happened; the last occasion being in 1833, when three eclipses of the Moon visible at Greenwich took place, as well as one of the Sun, about 9 digits in magnitude in this country and total in Iceland. Diagrams of all four will be found on referring to the well-known Francis Moore's Almanack for that year. Then, again, in 1768 and in 1722 this maximum number of lunar eclipses was more or less visible here; previously, in 1544, when three total lunar eclipses were more or less visible at London.

Melplash Vicarage,
Bridport, March 8.

Faithfully yours,
S. J. JOHNSON.

Canals on Venus.

GENTLEMEN,—

In one of your last numbers you have expressed your doubt as to the reality of the "canals" Mr. Lowell has drawn in his chart of Venus, and Capt. Noble did the same in a meeting of the R.A.S. Well, I am able to tell you that on March 24 I was so happy to see Mr. Lowell's canals on Venus, similar to his chart. The difference is only that Mr. Lowell took them for *real* features on the planet, while I found the justification of my suspicion that they are but *an effect of unsteady air.* Observing on March 24, when the air was 3, I made a drawing at 3^h G.M.T., which contained several of the faint, broad, diffused streaks one generally sees on Venus. Half an hour later the air had become unsteady, the image looked as though it were seen through heated air, and the disc now resembled very much the stereotypical representations of Mr. Lowell. An inexperienced observer would certainly have believed that he saw narrow dark canals, as distinct and sharply defined as the markings on the Moon. I waited until the image was calm again, and then made a drawing (at $4\frac{1}{2}^h$) with air 2; now, of course, *all Lowellian canals had disappeared*, the disc showed the usual streaks, but not the same outlines as an hour and a half before; and at $5\frac{1}{4}^h$ I made a third drawing with air 2–3 which shows a further change of the markings. To me it seems therefore an established fact, that Mr. Lowell's chart does not represent the markings on Venus, but the false images of his refractor. How it was possible that he was deceived in such a degree is explained in my paper 'The Rotation of Venus,' in the last number of the 'Journal of the British Astronomical Association.'

Yours faithfully,
LEO BRENNER.

Manora-Sternwarte,
Lussinpiccolo (Istrien).
1897, April 9.

[We print the above as a record of an observation by Herr Brenner, but we do not remember to have *expressed any doubt* as to the reality of Mr. Lowell's markings, nor do we now. We are not responsible for opinions mentioned in our reports of meetings. —EDS.]

OBSERVATORIES.

HARVARD.—The Annual Reports from the Harvard College Observatory have a praiseworthy sameness. Each year Prof. E. C. Pickering has to announce an immense catalogue of work done with the many instruments under his charge, and sometimes, it might perhaps be said often, to announce an addition to the equipment. In his last published report just received, which covers the year ending 1896 September 30, he remarks as the most important subject, in addition to the usual routine work, the completion of the Bruce Photographic Telescope, which, under the careful superintendence of Mr. Gerrish and Prof. Bailey, was taken, *viâ* New York and the Straits of Magellan, to Arequipa in Peru, where it was mounted and adjusted, and has been used to take 386 photographs. These are pictures of the sky on the same scale as those of the Astrographic Chart—viz., one millimetre to a minute of arc. Each plate covers a region five degrees square. Prof. Pickering expresses himself as quite satisfied with the result.

The record of work is a continuation of the records of former reports. The East Equatorial has been used for photometric observations of variable stars of the Algol type and of long period; also of the light-variation of Iapetus and Titan, and for photometric observation of Jupiter's satellites during eclipse, in continuation of the series made in previous years. The number of these observations now amounts to 599.

With the Meridian Photometer the Director has made 91,608 settings on 144 nights. This is the largest number of observations made with this in any one year hitherto. "About nine-tenths of the observations of all stars of magnitude 7·5 and brighter and north of −40° are completed." From observations with this instrument, Prof. Pickering has detected in U Cephei a small secondary minimum, with a deviation in light of about two-tenths of a magnitude. Also he infers that S Antliæ, which has hitherto been regarded as of the Algol type, is an ordinary short-period variable in which the decrease of light is more rapid than the increase. With respect to observation with this instrument, Prof. Pickering reiterates what he said last year, that it is more expedient to make many observations of any star quickly than to spend much time on each of a few.

With the meridian circle Prof. Searle has made 2061 observations for the purpose of revision connected with his reduction of the zone $-9° 50'$ to $-14° 10'$ of the Ast. Gesell. Catalogue, on which work he is now engaged. It will be remembered that the zone $+49° 50'$ to $+55° 10'$, which was allotted to Harvard, has already been published, and Prof. Rogers has nearly completed the printing of the observations on which this depends.

The other chief instruments of the Observatory are the 8-inch Draper spectro-photographic telescope and the Bache 8-inch photographic telescope. With the former 2508 photographs have been taken; with the latter, which is at Arequipa under the charge of Prof. Bailey, the record is 2770. All the spectra photographed have been examined by Mrs. Fleming, with the result that a large number of objects having peculiar spectra have been detected. These have been announced from time to time in the Harvard College Circulars. The list of these discoveries is as follows:—Twenty-one new variables discovered from the presence of bright hydrogen lines in their spectra, nine stars whose spectra is of the fourth type (two of these are included in the 21 new variables), three of the fifth type, one new gaseous nebula, one in which the hydrogen line $H\beta$ is bright, six of which the spectra are said to be peculiar, and the hydrogen-lines have been shown to be bright on the spectra of several known variables. Also, besides these, from examination of the spectrographs two new stars, Nova Carinæ and Nova Normæ, have been discovered, and from examination of the chart plates Miss Wells discovered five new variables, the most important of which is W Delphini, a star of the Algol class having a variation of 2·71 magnitudes. The Meteorological Observations have been continued at the various stations in connection with the Observatory. The station at Blue Hill, 12 miles south of Harvard College Observatory, which is maintained by Mr. Lawrence A. Rotch, but the results from which are published in the Harvard Annals, has been secured by a lease of ninety-nine years to the Harvard College authorities. It is recorded that at the Arequipa station a most violent earthquake occurred at five minutes past ten in the morning of 1896 June 14, which was about twenty hours before the great tidal wave in Japan which caused so much disaster.

WINDSOR, NEW SOUTH WALES.—Mr. Tebbutt's Report for the year 1896 opens with the statement: "The year 1896 having been characterized by a very large number of clear nights, I am enabled to present an unusually good report of work done." We are in the habit of reading astonishingly good reports from Mr. Tebbutt, but we think this present one does warrant the use of the epithet "unusually." Determination of time was made on 221 nights, which entailed the observation of 1495 transits of stars. The number of nights of observation shows that the weather was above the average. The number of observations of occultations of stars by the Moon, which is a special feature of Mr. Tebbutt's programme, amounted to 161, and is the greatest ever obtained by him in any year hitherto. "These comprise one hundred and thirty-two disappearances at the dark and eleven at the bright limb, and twelve reappearances at the dark and six at the bright limb. The results of these were sent to the 'Astronomische Nachrichten.'" Other work of this class was the observation of phenomena of Jupiter's satellites, 43 of which were made, which does not include the observation of the transit of two remarkable dark spots, and of some others across the central meridian. Besides these, with the 8-inch equatorial and the filar micrometer, Mr. Tebbutt measured the positions of eight of the minor planets, 810 comparisons in all, and also on seven evenings, snatched from other work, he observed a few double stars. The brightness of the variable stars R Carinæ and τ Centauri was observed on several evenings, and, speaking of the latter, the report says "its maximum occurred about March 8 with a magnitude of 5·3 of the scale of the 'Uranometria Argentina.' At the close of the year it was again approaching its maximum, the magnitude being estimated as 4·9."

Mr. Tebbutt mentions the investigation of the longitude of this observatory made by Dr. Hugo Clemens from the series of occultations of stars made in the years 1864–70, to which he referred in his letter in our last number. In the Report a list of all the best values, seven in number, previously obtained for this is given, and it appears that Dr. Clemens's value, $10^h\ 3^m\ 20^s{\cdot}60$, differs from the mean of these by only $0^s{\cdot}01$, which must be gratifying to that gentleman and to Mr. Tebbutt, who made the observations.

STONYHURST.—The greater part of Father Sidgreaves's Annual Report scarcely comes within the scope of this journal, but consists chiefly of meteorological and magnetical results given in a remarkably concise and intelligible form. The astronomical work of the Stonyhurst Observatory is probably well known to our readers, as this is usually communicated at an early opportunity to the Royal Astronomical Society. It may be sufficient for us to quote from the book for 1896 now before us that drawings of the Sun 10½ inches in diameter were made on 125 days (the dates and Greenwich Civil Time of each of these is given), that Solar Spectrum Photographs were taken on 139 days, and that "over 350 photographs of stellar spectra have been obtained with the compound prism spectrograph in combination with the Perry-

Memorial objective. These include some trials with the small dispersion of a single half-prism of aluminium glass in order to provide the means of learning the condition of the calcium line K in the spectra of small stars. The length of the spectrum is too small to show a fine line, but it distinguishes well between a broad, medium, and thin line in stars to the 6th magnitude."

PUBLICATIONS.

ANNALS OF THE PARIS OBSERVATORY OF PHYSICAL ASTRONOMY AT MEUDON, VOL. I.—M. Jannsen's first volume is to a great extent introductory. It begins with a concise sketch of the history of astronomical physics, showing how the science was inaugurated by Galileo's invention of the telescope, was set on a firm basis by Herschel and Arago, was enriched by the discoveries of Malus, Wollaston, Fraunhofer, Bunsen, Brewster, and others, and later has been led into much extended regions by the introduction of photography. M. Janssen apportions credit to different nations thus:—"In this work France may claim her share. If the telescope is Dutch by origin, Italian and English by the great discoveries made with it, photography belongs almost entirely to us; and if spectrum analysis has been given definite shape in Germany, and splendidly extended by the chemical analysis of the solar atmosphere, it is in France that the admirable method has received extension which has doubled its power by discoveries of the spectra of gas and vapours of the terrestrial atmosphere."

The next section of the volume deals with the establishment of the Meudon Observatory. The physical researches made in France in the years 1862 to 1871, and especially M. Janssen's great discovery with Prof. Lockyer at the eclipse of 1868, led to the idea of the creation of an observatory for such studies; and on his return from the Transit of Venus Expedition in 1874, M. Janssen found the project under discussion. In 1875 a sum of 50,000 francs was set down in the government estimates for the year 1876, for the establishment of such an institution, and this grant was continued for some years. The next step was to find a suitable building, and a chateau at Meudon, belonging to the State, which was in a condition of partial ruin, having been set fire to after the war, presented itself as suitable for the purpose. In November 1878 this building was handed over for this purpose, and later a sum of 1,035,000 francs voted for its restoration and equipment.

To this there naturally follows a description of the equipment as it now is, the chief features of which may be tabulated as follows:—(1) The great equatorial, carrying a visual telescope of $0^{m}\cdot83$ aperture and $16^{m}\cdot16$ focal length, and a photographic telescope of $0^{m}\cdot62$ aperture and $15^{m}\cdot90$ focus; (2) a reflector of one metre aperture and 3 metres focal length on an English equatorial mounting; (3) the equatorially-mounted telescope of $0^{m}\cdot135$ aperture, used for solar photography; (4) a portable meridian circle and some other small instruments, as well as a completely-equipped spectroscopic laboratory.

The results published in this volume are twelve magnificent photographs of parts of the solar surface showing details of granulation hitherto unexampled. M. Janssen prefixes to these a memoir on solar photography, and is good enough to let his readers into the secret of the process by which these excellent pictures are obtained, which he sums up in a few sentences, naming as essentials a proper kind of glass, principally flint, for the objective, the construction of the object-glass so that it shall be achromatic for the G line, a collodion giving a maximum sensibility limited to that same part of the spectrum, fineness of grain of the collodion, and considerable enlargement of the image.

This publication is presented in a handsome form, clear type, fine paper, large margins, and, besides the Sun pictures, is illustrated by views of the Observatory.

NOTES.

COMET NOTES.—'Bulletin Astronomique' for March contains an investigation of the orbit of Comet Swift (1895 II.) before 1884, by L. Schulhof.

The author had previously suggested the probable identity of this comet with that of Lexell, and requested astronomers to obtain as long a series of observations as possible, in order to determine the elliptical elements with accuracy. The observations made at Mount Hamilton extend over five months, and enable the elements to be ascertained with considerable accuracy, as follows:—

Epoch = 1895, August 25·5, Paris M.T.

$$\left.\begin{array}{rl} M = & 0^\circ\ 38'\ 24'' \\ \pi = & 338\ \ \ 4\ \ \ 9 \\ \Omega = & 170\ \ 18\ \ 16 \\ i = & 3\ \ \ 0\ \ 22 \end{array}\right\}\ 1895\cdot0.$$

$$\phi = 40\ \ 41\ \ 56.$$

$$\mu = 492''\cdot51.$$

Period = 7·2043 years.

The probable error of μ is considered to be 0″·5, or of the period some three days. Unfortunately, this uncertainty, small as it is, prevents the accurate computation of the perturbations by Jupiter back to 1779. For even in 1838 it is impossible to say within 40 days when the comet passed any assigned point of its orbit. Till the comet has been observed at another return, it can only be asserted that the identity (or at least the connection) of the two comets is very probable.

The comet may possible be visible in 1902, but it will be faint and in considerable S. declination. Should it not be seen then, there will be little hope of seeing it before 1931.

Ast. Nach. 3416 contains an interesting paper, by J. Holetschek, giving notes on many ancient comets, including several apparitions of Halley's Comet recorded by the Chinese. A. C. D. C.

MINOR PLANET NOTES.—Ast. Nach., 3414, contains a new determination of the orbit of (151) Abundantia, by Herr Groeben. It will be recollected that he recently made the suggestion that this planet had a companion travelling in an almost identical orbit, which had sometimes been observed by mistake for Abundantia, as several of the observations of the latter showed large discrepancies from the tabular places. Careful search last autumn at Munich, Berlin, and Düsseldorf failed to reveal such a companion, and Herr Groeben has therefore made a new investigation of the orbit, and now finds that, after recomputing the perturbations by Jupiter and Saturn, it is possible to represent all the observations with tolerable accuracy. A. C. D. C.

THE SOLAR MOTION AS A GAUGE OF STELLAR DISTANCES.—A valuable paper on the relative distance of stars of different magnitudes, which we regret not to have mentioned before, appeared in Ast. Jour. No. 390. It need scarcely be pointed out that the first assumption that occurs to anyone as to the distance of stars is that the brighter stars are nearer than the faint ones, or, carrying this to the extreme, that in the general mean the stars of the different magnitudes have nearly the same parallax. This hypothesis, which is as old as the time of the elder Herschel, has been used with different variations by various investigators, and has been found to have more or less foundation in fact. Recently Prof. O. Stumpe has found that the solar motion in space, measured in angular distance as seen from stars, differs little for stars of different magnitudes. Prof. Newcomb, in the paper above referred to, evolves a similar result, or, in his own words, "the effect of this solar motion diminishes less rapidly with stars of faint magnitude than has hitherto been supposed."

The data of this investigation are the proper motions in declination of 3231 stars found by Auwers from comparison of his catalogue with that of Bradley. These quantities are divided into four groups according to their magnitude, and then each group is arranged in order of the proper motion of the stars; thus a table is formed to show the number of stars whose P.M. in a hundred years lies between 0 and 1″, 1″ and 2″, 2″ and 3″, and so on, in both directions, from $-15''$ to $+12''$, all the stars outside these limits in each direction being included in one group. It might be expected that these numbers should follow the theoretical law of error; but they do not do so, and this, Prof. Newcomb argues, is due to the solar motion. He deduces a mean apparent proper motion of all the stars in each class which is due to this motion, which is therefore proportional to the mean parallax, and this leads him to the general conclusion above stated. The actual numerical values of the relative parallax found are as follows:—

Group.	Limits of Magnitude.	Parallax.
I.	1 to 3·4	1·85
II.	3·5 to 4·9	1·18
III.	5·0 to 5·9	1·00
IV.	6·0+	0·87

The paper concludes with the description of another method of determining the solar motion, which confirms in some degree the previous result. Prof. Newcomb was comparing the places of the stars common to the two Berlin zone catalogues made respectively by Dr. Auwers and Dr. Becker for another purpose, and in course of this work detected a sequence depending on the Right Ascension, which was perplexing until he noticed that Becker's observations were made ten years later than Auwers', and that the periodic term might be explained by the parallactic motion in this interval. These small differences of R.A. gave a solar motion towards a point in the celestial sphere of Right Ascension 297°, of centennial amount as seen from a mean sphere of stars of the ninth magnitude of 4″·6. The differences of declination are not so satisfactorily explained; but Prof. Newcomb promises a second paper treating the subject more fully, to which we look forward with some interest, as the subject is full of suggestion, and can scarcely be treated adequately in a short note such as this.

THE LATE M. D'ABBADIE.—The following is an extract from an obituary notice, by Col. Bassot, in the 'Revue Générale des Sciences':—"What characterized the work of d'Abbadie was his method of observation. This *savant* had recognized how little precision attached to independent determinations of geographical positions, that is to say those which are determined directly from observations of stars by field instruments, and of which the best are exact only to about one or two kilometres. He conceived the *géodésie expéditive*, which consisted in forming a network of triangles, more or less well proportioned, which included all the natural signal points that could be seen from the improvised stations on the heights where the party halted. This method gives a precision ten times greater than that of independent positions, and affords a valuable means of verification. D'Abbadie thus established in Ethiopia a network which included 8° 32′ in latitude and 3° in longitude, and furnished the positions of 857 points. These observations were made with a small theodolite, the instrument at that time the most practical and the most accurate for a journey, which he improved by placing the telescope parallel to the horizontal limb. This telescope, turning about an axis, can be pointed, thanks to the interposition of a prism before the object-glass, upon every object in azimuth and altitude. It thus constitutes a new instrument, to which he gave the name 'd'Aba.'

"Of very original mind, very eager, d'Abbadie did not confine himself to geodesy; he was equally devoted to astronomy. He went to Norway to observe the eclipse of the Sun in 1851, to

Spain to observe a similar phenomenon in 1860. He was chief of the expedition sent to the Antilles by the Academy of Sciences to observe the Transit of Venus in 1882.

"His château d'Abbadia, near d'Hendaye, which he has presented to the Academy, and where he spent six months of each year, contained a perfect observatory for the astronomical and physical studies that he followed. Here he investigated for a long time the periodic variations of the deviation of the vertical.

"His interest for all things pertaining to field geodesy led to the introduction in France of a new method for the rapid measuring of base-lines—the method Jaderin, which is of the greatest service in field operations."

Mr. R. T. A. Innes, of the Cape of Good Hope Observatory, who we know has a very complete catalogue of southern double-stars, with their dates of observation, which might be serviceable to Dr. See, writes to us as follows in reference to our announcement of Dr. See's discoveries of southern double stars in our issue, No. 251, page 143:—"You say that Dr. See finds *h* 3683 (=Lac 1583) is in rapid motion. This is not borne out by the published measures, viz.:—

h	1836	81°	3″·8	2*n*
Sydney	1879	80	3 ·4	1*n*
Sydney	1891	77	1 ·9	2*n*

Both components were observed at Cordoba in 1874 with a very similar result.

"The star *μ* Argûs was found to be double at Sydney by Mr. Russell so long ago as 1880 (see 'Double-Star Results,' 1881, or Mem. R. A. S. vol. 47), and measures published to 1891 show no change.

"I am pleased to say that recent observations here confirm the duplicity of *ψ* Argûs found by Dr. Copeland several years ago. It is still very close (0″·6 ±), but the angle shows a large difference."

The double-star *β* 883 is well worthy of attention just now. Dr. See suspects it to be a very rapid binary (about 5 or 6 years). It is very close to the Sun, and hence observations must be made at once. Its R.A. is $4^h\ 45^m\ 30^s$, N.P.D. 79° 10′, mags. 7 and 7, and distance 0″·3. There is a third star at 150° and 18″.

The star attacked by Prof. Burnham in 'Popular Astronomy' for April is OΣ. 215, for which an orbit with a period of 107·94 years has been previously obtained. The ellipse representing this has been laid down with the observed positions, which show a uniform repugnance to come near it. Indeed a straight line will represent the observations much better.

Prof. Burnham's paper shows that although we may consider this a physical pair, it should be erased from the list of binaries whose periods are approximately known. It so happens that this star has been well observed with the 28-inch at Greenwich. The measures decide the question as to its being binary; but the orbit bears no resemblance to that determined by Mr. Gore. The period cannot be less than 250 years.

ZONE-TIME SYSTEM IN FRANCE.—The following proposition has recently been submitted to the Chamber of Deputies: "The legal time in France and in Algeria is the mean time of Paris, retarded by nine minutes twenty-one seconds." An article in the 'Revue Scientifique' on the subject of international time, referring to the above proposition, remarks: "Reduced to this and stripped of all which could hurt the susceptibilities of the most delicate, one may hope that this projected reform will meet with the reception that it deserves, both by Parliament and the public; that is to say, the unanimous approval of *tous les hommes de progrès.*" It may be added that it appears that the Société Astronomique de France did not approve of even this modified proposal.

THE COMPANION OF SIRIUS.—In Ast. Journ., No. 399, Prof. H. S. Pritchett writes:—"On March 20 I observed the companion of Sirius with the 12½-inch equatorial of the Morrison Observatory. The night was one of the finest I have ever seen in this climate, and the companion was seen by my father and myself. A mean of three settings of the micrometer gave

"$p = 195^\circ$. s (estimated) $= 3\frac{1}{2}''$."

THE obituary for the month includes the name of Prof. E. F. von Haerdtl, Professor of Astronomy at Innsbruck. Born at Penzing, near Vienna, in the year 1861, he died a comparatively young man. His astronomical work was principally of a kind calling for elaborate computation. An investigation of the movement of Winnecke's comet, another of the terms of long period in the lunar theory, and another on Assyrian chronology are attached to his name.

THAT liberal patroness of astronomy, Miss Catherine W. Bruce, has provided a sum of money to enable the Astronomical Society of the Pacific to award a Gold Medal annually as a recognition of services to astronomy. The gift is international—that is, the medal is to be given to the person considered most deserving, without regard to nationality, and also without regard to sex.

M. BIGOURDAN has taken great trouble to make measures from which to deduce the parallax of the star 1822 Fedorenko. He was induced to do so by the large proper motion of the star when compared with those near, which are practically fixed. He deduces a final P.M. of $-0^s{\cdot}056$ in R.A., and $-0''{\cdot}105$ in Declination, and a parallax which is insensible.

VISITATION-DAY at Greenwich will be on Saturday, June 5. The dinner, as in recent years, will be held at the Criterion Restaurant, Piccadilly Circus.

M. R. **Radau** has been elected a member of the Paris Academy of Sciences, in the Section Astronomy, in place of the late M. Tisserand.

It is announced that an observatory is to be established at Odessa as a branch of the Russian Imperial Observatory at Poulkova.

Dr. **Karl Bohlin**, of Upsala, has been appointed Director of the Stockholm Observatory, in succession to the late Prof. Gyldén.

Prof. **Simon Newcomb** retired from the post of Director of the 'American Nautical Almanac' at the beginning of April.

From an Oxford Note-Book.

The following pretty little poem by Barry Pain appeared in 'Black and White' for January 23:—

The Gamblers.

What are the careless yellow stars
  A-scattered down the sky?
They are the golden pieces that
  On the card-table lie.

Day with her white hand sweeps them up
  Triumphant her disdain;
Night waits, and with sure patience wins
  Her losses back again.

Tired now, for that last game they look
  Which shall decide the play:
I wonder whether time will stop
  By night or day?

There is a story told of Le Verrier that when one of the Greenwich observers went over to Paris to make observations for the longitude Greenwich—Paris, he tested his capacity as a transit-observer by setting him to count seconds out of hearing of any clock; and was quite disgusted when, at the end of a minute, the human clock differed a few seconds from the mechanical. In a book called 'Curiosities of Clocks and Watches' (London, R. Bentley, 1866) I found the other day an account of a man who would have passed this severe examination with credit. Here is the extract, rather condensed:—J. D. Chevalley, a Swiss, had in 1825, at 66 years of age, acquired the art of keeping time like a clock. On July 14, 1823, he kept a definite time correctly, talking all the time, and being distracted in various ways; indicating the quarters of an hour by his voice. He said that he had "acquired by imitation, labour, and patience, a movement which neither thoughts, nor labour, nor anything else can stop. It is similar to that of a pendulum, which at each motion of going and returning gives me the space of 3 seconds, so that 20 of them make a minute; and these I add to others continuously."

A letter from a correspondent mentions an experiment which may not be new, but seems at least worthy of attention. He says:—"I have heard it said that the Crescent of Venus has been detected with the naked eye. I am doubtful about it; last night (April 5) I reversed the finder of my telescope, which magnifies about 10 times, and turned it on the crescent Moon. Of course this diminishes the diameter to about 3 minutes of arc, and although this was 4 times the diameter of Venus at the present time, the Moon's crescent was very difficult to see; in fact I don't think it could be seen if reduced to less than 2 minutes of arc."

MY correspondent is keenly interested in the diameters of planets, and has made many measures of them, especially of Venus and Mars. He remarks on the wide range of values assigned to the diameter of Venus, from the 16″·6 of Airy to the 17″·7 of Ambronn. It is certainly curious that so many people have been content to go on measuring diameters without endeavouring to apply any corrections for personality or even for irradiation. There is no doubt that owing to these disturbing causes, all diameters are more or less arbitrary; and without some vital change in method, promise to remain so. A few years ago it was hoped that the Moon's diameter might be determined from occultations of small stars during an eclipse; but the trials of 1884 and 1888 were very disappointing in this respect. Again in 1897, Dr. Otto Struve had an excellent scheme for getting the accurate ratio between the diameters of the Sun and Moon, by placing observers near the totality limits on the occasion of the Total Solar Eclipse in that year; but cloudy weather interfered. Given good observers in plenty, this scheme might be tried again—say in India, next January. It consists in noting the duration of totality when it only lasts a second or two, with accurate determinations of the latitudes and longitudes of the station. And a very accurate ratio of the two diameters should result; but of course it would be very expensive in time, money, and labour. Beyond these particular instances there have not been any special attempts to get true diameters that I know of. Will photography ultimately afford us the means of solving the problem?

THE following little incident is of historical interest in connection with this question of true diameters. In 1848 Airy wrote to Lieut. Stratford, then Superintendent of the 'Nautical Almanac,' complaining that Bessel's value of the Sun's diameter was still used in the N.A., although the diameter had been observed at Greenwich with an instrument as good as Bessel's for many years, and with an exhibition of a different value. Stratford replied that he would gladly alter the adopted values of the diameters of the Sun and Moon, if Airy would indicate what values he wished adopted. Airy gave corrections of +1″·84 to the Sun's diameter from 12 years' observations, and −5″·22 to the Moon's, and these were adopted in the N. A. for 1853. In the volume of Greenwich observations for 1853 it is remarked with satisfaction that the "mean error of N. A." as regards the Sun's diameter in R.A. comes out $0^{s}\cdot00$, "thus showing that the value assumed in the N. A. for 1853, which is derived from the Greenwich observations made with Troughton's transit, precisely satisfies the observations made with the Transit Circle." But this satisfaction at having obtained what looked like a real value was short-lived. In 1854 the error in R.A. was $+0^{s}\cdot09$, "a quantity singularly at variance with the results of last year": in 1855 $+0^{s}\cdot07$, and in 1856 $+0^{s}\cdot10$; while the errors for the vertical diameter were $+0''\cdot58$, $+1''\cdot29$, $+0''\cdot65$, and $+1''\cdot17$ respectively. In fact the observations of the years 1854 onwards gave a diameter very nearly the same as that abandoned in the N. A. for 1853!

Another correspondent sends some contributions to the collection of cuttings from contemporary literature:—

No. 1. In 'The Limb,' by X. L., published in 1896, p. 250, we read that at St. Petersburg in December "the Sun clambers slowly to the zenith."

No. 2. Selections from a magazine article of last year. "On Mercury the Sun comes up like an enormous new Moon" ..... "Mars has two moons revolving in different directions: one is not larger than a cheese, but flies through space with the speed of a cannon ball, regardless of the movements of the regular Moon."

No. 3. "Though not astronomical, the following shows the danger of misunderstood signs. In a book on S. Africa, a flooded river could not be crossed until a carpenter made a boat in which he crossed over, size of said boat being $9'' \times 5''$, outside $12''$, depth $3''$; find the size of the carpenter?"

"Prof. Lowe's wonderful new telescope with 91 lenses to probe Mars mysteries." Such is the heading of a long account in a newspaper kindly sent by Prof. Barnard, of a nonsensical scheme to make up a big lens out of a series of smaller ones. "The new telescope instead of one single lens will have a lens made of many smaller ones. Each lens will be six inches in diameter." There will be one centre lens, then a ring of six; then a ring of twelve, and so on up to the outer ring of 30. All these homogeneous lenses will be cemented in a sash or frame, the cement having the same coefficient of expansion as glass; and the compound lens will be 72 inches in diameter. The new telescope is the invention of Louis Gathmann, a Chicago inventor, who "maintains that the so-called disturbed condition of the atmosphere does not exist, and declares that the seeming disturbance is wholly a defect in the construction of our present large telescopes." He points out that the tremulous condition of the atmosphere is readily noticeable in the large glasses, but "is not perceptible in the smaller glasses. . . . There is no reason in telescopic science why we should not see the inhabitants of Mars, and see, too, well enough to pick up a pin on the Moon."

I am glad to hear that Dr. Lewis Swift, who is reported to have approved this monstrous scheme, has been entirely misrepresented; and has, in fact, been doing his utmost to dissuade the misguided people concerned from proceeding further, but without success.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 254. JUNE. 1897.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, May 14, 1897.

Sir R. S. Ball, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., and
H. F. Newall, M.A.

The Minutes of the previous Meeting were read and confirmed.

The President. Before we proceed with our ordinary business, I should like to inform the meeting of a step which was taken by the Council this afternoon: it will, no doubt, be guessed that this refers to the sad circumstances connected with the death of one of the oldest and most esteemed members of the Royal Astronomical Society, Mr. E. J. Stone, Radcliffe Observer at Oxford. Mr. Stone, who was well a week ago, died suddenly last Sunday, and some of our Fellows were yesterday present at his interment at Oxford. Mr. Stone had had a very distinguished career, beginning with the position of Chief Assistant at Greenwich Observatory, which he filled for many years; from there he went to the Cape of Good Hope, where he was succeeded by Dr. Gill, and for the last 19 years he has been Director of the Radcliffe Observatory. He is known by the valuable catalogue of Southern Stars which he made whilst at the Cape. He is known also by his celebrated discussion of the Transit of Venus, to which he devoted an enormous amount of care and attention. We who were permitted to work with him knew him as one of the most regular attendants at these meetings, we were all permitted to enjoy his friendship and intimacy, and we all valued him as an able and experienced colleague. The Council to-day passed the following resolution, which I am sure the meeting will be glad to hear and with which I am sure the Fellows present will be glad to be associated:—"The Council, having learned with deep regret of the death of Mr. E. J. Stone, Radcliffe Observer, for 36 years a greatly esteemed and distinguished Fellow of this Society, who was, moreover, for 26 years on the Council, and had filled the offices of Secretary, Vice-President, and President, and had received the Gold Medal of the Society, desire to put on record their profound sorrow at the loss which Astronomy has sustained, and to express their condolence and deep sympathy with Mrs. Stone and other members of the bereaved family, and that a copy of this resolution be sent to Mrs. Stone."

The Fellows present expressed their approval of this resolution.

Mr. Newall. Since the last Meeting 78 presents have been sent to us, many of them being additions to series of presents already received, which make the library of the Society so valuable. Amongst them I may call attention to a book by Professor Holden on Mountain Observatories in America and Europe. There is also a fifth contribution from the Government of the Netherlands on "The Triangulation of Java," a work which has been in the hands of Prof. Oudemans for some years. Many of us heard him when he gave an account of this triangulation at one of our meetings. I further call attention to a contribution from the Lick Observatory which takes the shape of plates forming an atlas of the Moon, and also three prints of nebulæ and star-clusters taken with the Bruce 24-in. telescope of the Harvard Observatory; a work on the great Earthquake in Japan of 1891, presented by Prof. Milne; and, lastly, I will mention a photograph of Jupiter presented by Mr. G. F. Pollock—one of the first taken by Dr. Warren De la Rue. This is a glass transparency. Mr. Newbegin has also presented three photographs of Sun-spots which he has taken this month.

A vote of thanks was passed to the donors.

Professor Turner then read a paper by *Dr. T. J. J. See* on a new Binary Star with a period of 5½ years (β 883). The author said that for several months he had been practically certain that the double star β 883 was revolving in a period of 5½ years, and thus described its orbit in less than half the time of any star previously known. This hypothesis first presented itself to his mind in 1895. Schiaparelli has communicated to him his observations of this double star, and these bore out the fact that it was revolving in a period of 5½ years.

Prof. Turner then described the orbit on the blackboard.

Prof. Turner then read a paper by *Mr. Burnham* on the Orbit of μ Draconis. The author pointed out that the orbit of this star was not very well determined. He described the early observations of the star which was known as Σ 2130. Systematic work on this star was begun by Struvé in 1838, and from the early observations an orbit had been computed, but it was obvious that the data were altogether insufficient to determine even the roughest apparent ellipse. The chances were that the relative change of the two components would finally resolve itself into orbital motion.

Capt. Noble. I should like to make one remark in connection with these papers. In that first alleged orbit of Dr. See's it puzzled me for the moment to see that the principal star was on the minor axis instead of being at the focus.

Mr. Lewis. I should like to emphasize the work done by Dr. See on double stars, because he is now in the southern hemisphere doing a great deal of work which is very much wanted. With regard to β 883, we unfortunately only got a letter from him at the end of April, at which time the object-glass of the 28-inch refractor was reversed for photographic work. The Astronomer Royal, however, at once had the crown lens turned and the star was observed on April 23 and 24. As the declination of this star is now very much the same as that of the Sun, and it is only 2 hours after the Sun in R.A., it was near the horizon before we could observe it. However, on 2 nights we did get a look at it, and such observations as we were able to make quite bore out the present position of the companion to the star as given by Dr. See, and I do not think there is any doubt that it has a $5\frac{1}{2}$-year period. With regard to what Capt. Noble says, it is the apparent orbit that is put on the board, and not the true orbit. We are looking at this orbit almost end on. It is curious that all the apparent orbits of small periods are very similar. δ Equulei, Σ 3121, and β 883 might almost be interchanged. Before this, the shortest period star was $11\frac{1}{2}$ years, so that we have now got it down from $11\frac{1}{2}$ to $5\frac{1}{2}$, approaching closely to the spectroscopic binaries. With regard to Mr. Burnham's paper, he is doing good in showing the fallacy of many computed orbits. It is tending to land us down on bedrock as to our knowledge of binaries.

Mr. Seabroke. It does not do to attempt to construct orbits from too few observations. I think that many of our early orbits have this defect. That is the moral to which Mr. Burnham points. β 883, as Mr. Lewis says, is a most remarkable case of a binary; in fact, Jupiter is not comparable with it for shortness of period.

Prof. Turner. The admission by Mr. Seabroke is a very valuable one, as confirming Mr. Burnham's opinion that we have trusted to too few observations hitherto, and the astronomical world will be interested in that. I should like to ask if I understood Mr. Lewis correctly. Mr. See, in his first letter on the paper, says that it revolves in $5\frac{1}{2}$ years, which is the smallest period hitherto known. If I understood Mr. Lewis, he was classing two other stars with this one.

Mr. Lewis. Only with reference to the similarity of the apparent orbits. κ Pegasi and δ Equulei, the two shortest periods hitherto, revolve in $11\frac{1}{2}$ years.

Prof. Turner. I did not understand whether you agreed with what Dr. See said as to the previous record being $11\frac{1}{2}$ years.

Mr. Lewis. Yes, that is the previous record.

The President. These papers are of much interest, and that of Mr. Burnham is an awful warning of what may result from founding conclusions on insufficient evidence.

Capt. Hills (reading a paper on the determinations of terrestrial longitudes by photography). Some two years ago I presented a short note to the Society, giving the results of some experiments on this subject. The work, which is now brought forward, has not been confined to mere observatory work, for advantage was taken of the dispatch of a survey party to the centre of Africa—to the sources of the Niger—in order to test the work actually in the field, and by a discussion of the plates then obtained I have gained much valuable information. The work done on this expedition does not give much information as to the accuracy to be expected, because there is no check on the results. The value of the method must depend upon two lines of research: first of all on the practicability of it as a method in the field, and secondly upon the accuracy of it from the work done at home at a fixed station whose exact longitude is known. The great advantage that a photographic method has over a visual one is that the work done in the field is reduced to a minimum, and the observer needs very little skill. All the work is done afterwards at home in the measurement and reduction of the plates, and the only skill required in the field is a reasonable amount of care in the observations and an ability to determine the local time, which is one of the easiest of astronomical observations, but some objections have been made to this method on the ground that the results are not at once available.

It is a question which may be investigated as to whether it is not possible to measure the plate in the field, and thus get the results at once. That question has, however, not been tackled, because it is a large one, and I have confined my attention to determine what measure of accuracy we can get by the means available and by using a suitable instrument. [Capt. Hills then described in detail the instrument and the method of the observation, which consists in photographing the Moon and known stars on the same plate, noting accurately the time between the exposures, and keeping the camera rigidly fixed during the interval.] It is immaterial whether the stars are photographed before or after the Moon; the essential point is that we should have the images of at least two stars on the plate, and it conduces to accuracy to have stars both north and south of the Moon, though this is not absolutely essential. The necessary interval between the exposures may amount to a considerable time in certain cases, which depends upon the nearness of the star, which is of sufficient magnitude, to give a measurable trail, and that again depends upon the state of the atmosphere. There was generally no difficulty in getting a photograph of a star not too near the Moon. I do not consider that the ordinary tripod-stand for the instrument is sufficiently stable. The ends of the legs of the tripod should be flat in order to give a high degree of stability. The longer the focal length of the instrument the greater the accuracy, but a longer telescope is necessarily heavier. The observer must therefore determine for himself where he will strike the balance.

The measurement of the plate (which would be done at home) is a subject which has received a large amount of discussion in connection with the Astrographic Chart. I have tried two different methods of measuring. One is to photograph a positive réseau, and put this in contact with the plate to be measured; the other is to rule the lines on the plate and measure from these with a simple screw-micrometer. The advantage of putting the lines on the plate was that the plate could always be re-measured if there was any doubt as to the accuracy of the first series of measurements. The only difficulty in the measurement is that which arises in determining the Moon's centre, and this is one of the principal difficulties of the method. It is easy to get the coordinates of the limb, but it is necessary to find the coordinates of the centre, and I have done this by a method which practically amounts to drawing a pair of tangents at right angles to the limb and then measuring the distance of their point of intersection from the limb.

For computing the position of the Moon from the measured coordinates I have used the method of rectangular coordinates devised by Prof. Turner, which is used for the Astrographic Chart, and which, in fact, makes my method possible. It will easily be realized that the longitude may be determined in one of several ways, either by the Moon's change of declination, of azimuth, of distance from a star (commonly called lunar distance), or by its change of Right Ascension. The change in declination is sometimes so small as to be of no value; the azimuth is rather a difficult computation, and the change in lunar distance is almost the same thing as the change in R.A., but a little more complicated. The Right Ascension method seems to be the best, and seems to give some precision of result.

Prof. Turner. Capt. Hills has very much simplified one way of determining longitudes. There are two methods of determining longitude, which may be roughly distinguished by the fact of the existence or want of a telegraph-line. The determination of the longitude consists of two operations—the finding of the local time at the place and getting the time from Greenwich by telegraphic signals, or by finding Greenwich time and local time, and by comparing these the longitude is found. When there is a telegraph-line the finding of the local time is the chief difficulty. Capt. Hills referred to this rather contemptuously as one of the easiest of astronomical observations; but I think I may say that on occasions when two independent sets of observers have tried to determine this they have generally differed. Then when we come to the other method of determining longitude, *i. e.* when the telegraph does not exist, the obtaining of Greenwich time resolves itself into some method of observing the Moon. It is this operation which Capt. Hills has, I think, by his recent work very much simplified. He has not, so far as I know, touched the question in a way which may interest sailors. His present method of a fixed camera does not apply to observations of the Moon from the deck of a ship. There is a story told of a man who was once asked the question as to how to find the time at sea, and the reply began:—"First you set up the transit instrument on deck." (Laughter.) But the days are not yet gone by for Moon-culmination observations. I think that Capt. Hills's method of setting up a fixed instrument in the absence of the telegraph is of great interest to astronomers from the point of view of fundamental astronomy. I remember that the late Father Perry was kept on Kerguelen Island for a considerable time longer than he otherwise would have been owing to the want of a method of determining longitudes, and if the photographic method had been in existence at the time, some months of privation might have been avoided. After the Transit of Venus of 1874 had been observed, Father Perry found that a sufficient number of Moon culminations had not been obtained, owing to the bad weather and other causes, to enable him to assume that the longitude had been determined with accuracy. He therefore determined to stay there a sufficient number of months to secure the number of observations which the then Astronomer Royal, Sir George Airy, had instructed them was necessary. I cannot but feel that that study of photographic methods is bound to help us considerably in the near future.

Mr. Dyson. Judging from the results, it seems to me that the accuracy Capt. Hills has obtained is very remarkable, because he has found differences ranging within a second of longitude, which corresponds to a second of arc on his plate, and I think that is an exceedingly accurate result to obtain.

Capt. Noble. I think geographers may congratulate themselves upon the devising of any methods which will supersede the method by Moon-culminators. I once made a series of observations myself, and reduced them, and I think that very few of them came out anything like in accordance with what I knew to be the longitude.

The President. I am sure we are all very much obliged to Capt. Hills for his interesting paper. It is certainly an improvement in the practical method of obtaining longitudes, which is very important. I have lately been reading Nansen's book, particularly that part in which he says that one of his woes was that his watch had run down and that he was utterly at sea as to his longitudes. Now, if he had had some commodious way of determining his longitude, he would not have had to spend a whole winter on one side of a hill while the relief expedition was on the other.

The Astronomer Royal then exhibited some photographs of the Moon taken with the new Thompson telescope at Greenwich. One was taken in the primary focus, the image being 2½ inches in diameter, and on one of Hill-Norris's dry collodion plates. Another photograph shown, taken on Wednesday, May 12, was magnified 4·6 times in the telescope, giving the diameter of the Moon as 11½ inches, corresponding to 104 feet focal length. The magnification was obtained by one of Dallmeyer's tele-photoconcave magnifiers. The enlarging camera and the lens were the same combination that he took out with him to Japan for the eclipse, to be used in combination with the Thompson 9-inch telescope, and it was used here with the Thompson 26-inch. He trusted that they would be able to get results better for exhibition purposes. It was a very interesting question as to what was the best process for obtaining photographs of the Moon. On the one hand, if they took a photograph on a small scale in the primary focus, having abundance of light they could obtain, with a slow process and a comparatively short exposure of not more than 2 seconds, a good photograph; so that these dry collodion plates, which had a very fine grain, gave the possibility of great detail. Under ordinary conditions, unless special arrangements could be devised, it was hardly practicable with the Thompson telescope to obtain a picture with an exposure of, say, 20 seconds, on account of the Moon's motion in declination, and they were, on May 12, limited to an exposure of 2 seconds; but even in that time the Moon would have moved half a second of arc in declination. There was no means of moving the plate. It seemed to him to be rather a balance between coarseness of the grain and the rapidity of the plate. If very rapid plates were used they had a coarse grain, which counteracted the advantage of direct magnification at the telescope. If dry collodion plates were used they would require a much longer exposure, and practically these could only be used at the primary focus. Results from these were very beautiful when they succeeded in obtaining them, but it was not always that they got them, as these plates were extremely variable in their action. It was a question, however, that required working out.

Dr. Common. I have looked at the negatives of these photographs, and the detail appears to be enormous. As to the question whether it is best to take a small image in the primary focus or an enlarged image, I should say that the better solution is to take the large image with a slow plate, using mechanical means for moving the plate in declination. Dr. Draper used a sliding frame for exposing his wet plates, and, so far as I recollect, it had not a motion of rotation. He had a sand-clock which regulated the moving of the plate in the line of the Moon's motion. He had a fixed telescope, and he was wise in that. We must go back to that, or to some similar plan, in order to get details. It has occurred to me that a simple plan would be to have a rotating plate. The question is, however, how to correct for the varying rate of the Moon. If the motion is correct for the maximum rate of the Moon, the exposure can be continued for 20 or 40 seconds without appreciable error. I think that if the Astronomer Royal would apply something of that sort to his telescope he would be able to take a large picture with either a wet or a dry plate and obtain some very magnificent results.

Capt. Hills. Does not all this point to the fact that the equatorial mounting is not the best, because if we take the form of mounting like the coelostat it is much easier to correct for all forms of motion? It would be comparatively easy to drive the mirror so that it should follow the motion of the Moon.

Mr. A. Taylor. What would the size of the coelostat mirror have to be to use with the 26-in. telescope?

The Astronomer Royal. There is another solution of the question preferable to making the plate slide. The Thompson equatorial at Greenwich is provided with very delicate slow motions and with a powerful guiding telescope, and I should be inclined to try to use the guiding telescope, by keeping a lunar spot as perfectly as possible on the cross wires. That has actually been done with the Astrographic telescope. An exposure was made for 20 seconds on the Moon with hand-guiding, where the means of guiding is not so delicate as in the case of the Thompson equatorial. I think, considering the smaller size of the instrument, that the photograph obtained, which shows Copernicus, is very good. It is feasible by hand motions to obtain a fair photograph. We have means of accelerating or retarding the rate so as to equalize the clock with the rate of the Moon. As regards the motion of the Moon in declination, this photograph was taken under most unfavourable conditions. There are two ways of getting over the difficulty—we may either take the Moon when the variation of declination is small, in which case the exposure can be extended for a time; or we might apply some clockwork arrangement to the slow motion in declination, and this would be better than giving a sliding motion to the plate, which involves a good deal of trouble. It must be remembered that we do not propose at Greenwich to devote the Thompson equatorial to the endeavour to securing the very finest photograph of the Moon that can be obtained. There are so many other important uses to which photography can be applied. My own predilections are in favour of working in what I look upon as the more useful field of double stars and small objects of that sort. In ordering the instrument I was careful to stipulate that the slow motion in declination should be extremely fine. We could, if necessary, apply clockwork, or the telescope could be moved in declination by hand. I should be disposed in the first instance to try the hand control.

Mr. A. Taylor. I think Schaeberle in America is trying this form of hand-driving with a Cassegrain telescope, and he says he can get exposures as long as 45 and 60 seconds without any great movement being shown on the film, so that the method, if it can be carried out, would seem to be a most promising method for obtaining a photograph of the Moon. It is interesting to know that at Paris they have adopted the sliding-plate method in preference to attempting to move the very heavy equatorial.

The President. We have the privilege to-night of welcoming a very distinguished visitor, namely, Mr. Swasey, of the United States. He is a man of great eminence, and an engineer who has devoted his skill to making mountings for astronomical instruments, and the firm with which he is connected has been putting up the mounting for the Yerkes telescope: we shall be glad if he will favour us with some remarks on his line of work.

Mr. Swasey. I am very pleased to meet you this evening, and am very gratified at the kind expressions of your President with regard to the work with which I have been connected. With regard to the great equatorial which has recently been constructed, although it is not yet quite complete, it will probably be in working order within two or three weeks. I rather hesitate to give any figures with regard to that instrument because I know they are disappointing. It is an extremely large instrument and is the most powerful refracting telescope yet constructed, although there are others, I understand, in process of construction. The aperture is 40 inches in diameter and the focal length 62 feet. The instrument, when completed, will weigh about 70 tons, and I may say that when we get to that size it gets past instrument-making—we then get to engineering. The cylinder, 62 feet long and 4 feet in diameter, weighing, with its accessories, about 8 tons, has to be as easily handled as a smaller equatorial, and therefore we have some elements of difficulty to overcome. When the clockwork moves the instrument in R.A. it has to move 20 tons—the hands of the clock weigh 20 tons—so that we have to prevent all the friction we possibly can. I might continue to mention weights and diameters in this way, but I will not trouble you. The instrument is designed for visual work, and no arrangement has been made for photography. The design is similar to that of the Lick telescope, the general feature being a rectangular column surmounted by a cast-iron head. The rectangular column is made very heavy indeed, and that is the basis upon which the whole rests, and this is set upon masonry foundations. The motions of this great instrument are controlled principally by electricity. In the Lick telescope they are entirely controlled by hand, but it was found in moving such a heavy mass that it was well to apply electric power to all the fine motions, so that the large motion in R.A. and in Declination and the slow motion in R.A. and Declination are all operated by motors. The wires controlling these motors all come to the eye-end and are operated by the astronomer, or to the balcony to be operated by the assistant, and so, to use a phrase familiar in America, "the astronomer presses the button, and we do the rest." The eye-end is supplied with a micrometer designed in accordance with Mr. Burnham's ideas, and as we all know what excellent work he has done in that line, we were only too ready to follow his designs.

Once when talking over the construction of the micrometer with Mr. Burnham we suggested that it would be well to use electrical illumination for the cross wires, and Mr. Burnham said that a good oil-lamp was good enough for him; but he said, "When I am gone, perhaps some one else would like the electricity, so it would be well to have both." So the micrometer is to be illuminated in both ways. I know you all agree with me in hoping that it will be long before the electricity is used. Mr. Burnham is to have charge of the instrument for a portion of each week, and Professor Hale will work with the spectroscope, and Mr. Barnard, who has recently been honoured by this Society, is also to use the instrument. About the last-named astronomer, I want to say that those who are especially interested in his works and those who know him personally have nothing but the greatest praise for this Society for bestowing upon him this honour. We all love Mr. Barnard, he is such a genuine, noble man, and when he is honoured, we are all honoured. To refer again to this telescope, such a large instrument requires an apparatus to enable the astronomer to accommodate himself to all positions, for when it is pointed to the zenith the object-glass is nearly 70 ft. from the floor, so that an elevating floor is a necessity. That is 75 ft. in diameter and has a rise and fall of about 25 ft. This floor is operated by an electric motor, and is controlled at will by the astronomer. The dome, which is 90 ft. in diameter, and weighs 200 tons, is revolved by electricity, and that is under the control also of the astronomer. As I have said, the instrument has not yet been put into operation, but I think it will be within a few weeks. As to the object-glass, which has been made by Messrs. Clark, and weighs about 1000 lbs., the transporting of that object-glass to Geneva Lake, 100 miles N.W. of Chicago, will have to be done with great care. The distance it has to traverse is 1200 miles, and it has been quite a problem as to how it can be carried in safety. One of our Pullman sleeping-cars has been chartered; the glass will be carefully packed, and so taken great care of.

The President. I am sure we have all heard these remarks of Mr. Swasey with the greatest possible interest. It is very good of him to have given us the opportunity of hearing from the lips of the maker himself some account of this wonderful machine.

The following papers were announced and partly read:—

W. F. Denning. "Zodiacal Radiants of Fireballs."

T. J. J. See. "On a new Binary Star with a Period of five and one half years (β 883)."

David Gill. "On the Mean Places and Proper Motions for 1900 of 24 Southern Circumpolar Stars."

Capt. E. H. Hills. "On the Determination of Terrestrial Longitudes by Photography."

S. W. Burnham. "The Orbit of μ Draconis."

Radcliffe Observatory, Oxford. "Observations of Comets Perrine 1896 Nov. 2, and Perrine 1896 Dec. 8."

W. E. Plummer. "Cometary Observations made at the Liverpool Observatory, 1896."

The following gentlemen were elected Fellows of the Society:—

Arthur W. Clayden, M.A., F.G.S., F.R.Met.Soc., F.C.S., Technical and University Extension College, Exeter; *Herbert Lovis Noel Noel-Cox,* 2 Edinburgh Mansions, Victoria Street, S.W.

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE sixth ordinary meeting of the seventh session of the British Astronomical Association was held on Wednesday, April 28th, at Essex Hall, Essex Street, Strand, the President, *Mr. N. E. Green, F.R.A.S.*, in the Chair.

Seventeen new members, thirteen of whom will be connected with a proposed new Branch of the Association in Melbourne, were elected, and the names of sixteen Candidates for Membership were read and suspended.

A paper by *Mr. Edwin Holmes* on the "Rotation of Venus" was read. The writer after alluding to the two opposing views of the rotation period of the planet pointed out that the advocates of each period arrived at their conclusions by ignoring all observations that conflicted with their own views. Mr. Holmes believed that the actual observations on both sides were trustworthy, and that no theory could be regarded as satisfactory that did not reconcile them. Mars had been usually regarded as the planet having the greatest resemblance to our Earth, but the writer contended that he was almost certain that Venus was more a counterpart of the Earth than any other planet. All evidence pointed to a heavily cloud-laden atmosphere. There was no sharp configuration as on the Moon or even on Mars, although Venus was more brilliantly lighted and the contrast between peaks and shadows should be more intensely marked. If, then, they admitted that they never saw the true surface, but mainly the scattered reflection of sunlight from clouds, and that the less bright places were simply places where the clouds thinned out more or less, and allowed indistinct vision of the darker body beneath, they would have to record the Scotch verdict "not proven" concerning every alleged determination of the period of rotation. This would be a confession that they knew no more about the rotation period of Venus than when J. D. Cassini undertook to investigate it more than 200 years ago, but it would be in harmony with the opinion of one of their keenest observers, Schiaparelli, who had stated with regard to the hypothesis that the spots were permanent formations, that it "is perhaps as imaginary as the said periods, which no one has proved up to now."

A paper on the same subject by *Mr. Henry MacEwen*, Director of the Mercury and Venus Section, was next read. The Director stated that the members of the Section were divided in opinion on the question. Some favoured a period comparable to that of the Earth, some a long period, and others had failed to detect any reliable evidence on the question. The opinion of Herr Brenner, a member of the section, is well known, and is given by him with great exactness as $23^h\ 57^m\ 36^s$.

Herr Brenner claims to have seen the planet turning round. The Rev. T. H. Foulkes, as the result of a series of observations made at Devonport, came to the conclusion that it was impossible even to suggest what Venus did in the matter of rotation, but he was strongly of opinion that the rotation period must be short, say 24 hours ±, and that a period of several days was quite out of the question. The Director himself was at one time of this opinion, but since 1895 he had come to the conclusion, from a careful discussion of all available drawings back to the time of Cassini, that a rotation period of 224·7 days best agreed with the evidence. He himself had never seen any sign of rotation of the planet.

Mr. W. H. S. Monck, in a paper on the same subject, doubted the reliability of drawings of very faint markings, and suggested that an arrangement should be made for drawing the planet at the same hour at different stations and by different observers.

Mr. Maunder thought that there was a good deal in the point raised by *Mr. Holmes* in the beginning of his paper. If they compared Venus and the Earth together as to mass and size they found them to be the two planets that most resembled each other of the whole solar system. They would naturally, therefore, expect Venus to have an atmosphere not differing greatly from our own. But it was pretty certain that observers on Mars or Venus would be able to distinguish little, if any, of the geographical features of our planet. It was, however, exceedingly probable that, assuming a period of about 24 hours for Venus, atmospheric currents might be set up on that planet which would have the appearance of faint markings, which would necessarily hold a definite relationship with the terminator, and would therefore give rise to the idea that Venus actually rotated, not in 24 hours, but in 224 days.

Mr. G. F. Chambers thought that there had been too much dogmatism with regard to the markings of Venus and the conclusions which had been drawn from the observations of those markings. He was bound to say that, using a very good 6-in. Grubb refractor, he had been able to make very little out of the planet, and he believed that they would never be in a position to draw any safe and sound conclusions about the planet until it had been subjected to a long course of observation with some of the very largest telescopes in existence.

Mr. Wesley observed that, assuming, as *Mr. Holmes* had done, a similarity of atmospheric conditions between Venus and the Earth, they would expect, if the rotation of Venus was a rapid one, to see some trace of belts caused by trade-winds.

The President wished to throw out one or two hints with regard to the use of telescopes in difficult seeing. The edges of either an object-glass or a mirror were the parts where the difficulty arose, and the use of a stop, even a small stop, under such circumstances would be found a great advantage. On one occasion he had been greatly puzzled by the behaviour of his 18-in. reflector, and had tried stop after stop without success, until at last he had stopped down to about seven-inch opening, and then he got a satisfactory view. Of course, under such atmospheric conditions a small telescope would do work as good or better than a large one.

Mr. Griffiths said that he had observed Venus on some twenty nights, and on every occasion, if definition was good, he had been able to see some markings. He had observed the planet for 3 hours on one occasion, but he had never seen any turning. The markings never moved at all from the positions in which he saw them, with regard to the terminator; as the crescent narrowed they moved with the terminator towards the limb. He had never seen anything that gave the idea of dense clouds, although some of the markings suggested a slight mist.

The second interim report of the Mercury and Venus section was read, and a number of photographs and drawings of Venus—the former by *Mr. Offord*, and the latter by *Mr. Foulkes*—were exhibited.

Papers by *Mr. Monck* on "The Limits of the Universe," by *Herr Brenner* on "The Rotation of Uranus," by *Mr. William Godden* on "A Naked-eye Observation of Mercury," and by *Mr. S. E. Peal* on "The Dark Tints of the Lunar *Maria*" were read.

Herr Brenner's paper was accompanied by 12 drawings, and the conclusion he had arrived at was that Uranus rotated in about 8¼ hours. Twenty drawings of Mercury by Herr Brenner were also shown, which, he contended, proved the impossibility of a slow rotation period beyond doubt.

The President announced that Miss Brown, the Director of the Solar Section, had made the Association a present of £50 towards the cost of erecting the new observatory on the site offered by the Royal Botanical Society.

Mr. Petrie mentioned that the President had offered his fine 18-in. reflector for the Observatory, and that Mr. Calver had also offered optical aid.

The death of *Mr. Adam Hilger*, a member of the Association, was mentioned, and a resolution of regret and condolence with the widow and family adopted.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday afternoon, the 19th May, at the rooms of the Royal Astronomical Society, Burlington House, *Mr. E. Mawley, F.R.H.S.*, President, in the Chair.

Mr. F. Gaster, of the Meteorological Office, read a paper by himself and *Mr. R. H. Scott, F.R.S.*, on the mean monthly temperatures of the British Isles. The authors dealt with the means of the daily minimum, average, and maximum temperatures for the various months of the year in the 25 years 1871–95. They pointed out that there is a great difference between the amount of range of temperature at the coast stations and that recorded inland. The range between January and July amounts to about 16° at coast stations, but to more than 23° at the inland stations. The contrast between the temperature of the air at inland and at coast stations at different times of the year is due to the following causes:—(1) The constant tendency of the Sun to heat the surface of the Earth; (2) the equally constant tendency of the Earth to radiate its heat into space—both of these being modified greatly by the aqueous vapour and the clouds suspended in the atmosphere; (3) the fact that the solid portions of the Earth absorb and reflect heat much more rapidly than the water; and (4) that while the ocean to the westward is of enormous size and great depth, the sea to the eastward is, comparatively speaking, limited in area and shallow, and separates the eastern shores of the British Islands from those of continental Europe by a small distance.

A paper by *Mr. C. V. Bellamy* on "The Rainfall of Dominica, West Indies," was also read. The author gave an interesting account of the climate of the island, and then discussed the monthly returns of rainfall from 27 stations during the four years 1893–96. The rainy season extends from July to November, the other months representing the dry season. The month of November 1896 was the wettest on record.

Edward James Stone.

WITHIN the last year several vacancies have appeared with terrible suddenness in our astronomical ranks. We were not in any way prepared to lose Gould or Tisserand, and from among those whose fame was not so world-wide, but whose faces were familiar to English astronomers, George Hunt and Sidney Waters have vanished. Now with the same lack of warning we miss Stone. It is difficult to realize: he was so active and energetic and looked so well and strong, in spite of his 66 years. Many men a dozen years younger would have hesitated to go to Nova Zembla last year, and one would not have wondered at their hesitation; but there did not seem anything remarkable in Stone undertaking the expedition. His age sat so lightly upon him that it was a positive effort to think of him as nearing the "three score years and ten," and without any incongruity he ranked as a man in the prime of life.

He was born 1831, February 28, the elder son of Edward Stone of London. The family came from Devonshire. He went up to Cambridge late in life, according to the usual standard; but his higher education began only when he was 20, and under the circumstances his scholarship at Queen's in 1856, and his degree as fifth wrangler in 1859, are somewhat remarkable. In 1860 he succeeded Main as Chief Assistant at Greenwich—as he afterwards succeeded him in the appointment of Radcliffe Observer at Oxford. By a curious series of coincidences, Main and Stone both went up to Cambridge rather late: to the same College (Queen's); were sixth and fifth wranglers respectively; were both appointed Chief Assistant at Greenwich within a year of taking their degrees; were Radcliffe Observer for 19 years each; and both died on the 9th of May.

In turning over the Astronomer Royal's reports for the years 1860–1870, during which Stone was Chief Assistant, I came across various references to him, of which the following is worth quoting. It was (so far as I know) unusual in Airy to subordinate his own judgment to the extent indicated, and the extract is a testimony to Stone's force of character. It is from the report of 1870:—"We have lately begun to act on the system of using, for the reduction of each night's observations, the Nadir Point and the Correction for Runs determined on that night. I have for the present adopted this system at the suggestion of Mr. Stone. It is to be remarked that, while on the one hand it makes each night independent of the faults of all other nights, on the other hand it leaves all results affected by the one unbalanced error of the one determination. My own judgment is in favour of using the mean of several nights' determinations when there is no obvious sign of instrumental change. It is, however, a point on which experienced men may well differ."

While at Greenwich, Stone rediscussed the observations of the Transit of Venus in 1769, and for this work obtained the Gold Medal of the Royal Astronomical Society in 1869—just a hundred years after the transit.

There is no doubt that Stone worked very hard at Greenwich, as indeed throughout all his life. It is common knowledge that he was sent to the Cape to do a piece of hard and tedious work, to make, in fact, the Cape Catalogue, which he did make. Our knowledge of the southern hemisphere was getting more and more into arrear, and vigorous measures were required if the matter was not to become hopeless. By ten years' hard work Stone produced the Cape Catalogue of 12,441 stars (Epoch 1880). The impression conveyed by these figures to the majority, even of astronomers, is probably not a very definite one; but if they will accept the word of those who have had experience of this kind of work, they will give all concerned in the production of this Catalogue credit for unsparing devotion and patience. By way of illustration (though there are many circumstances to be taken into account impossible to specify here), I may say that the Greenwich Ten Year Catalogue for 1880 contains only 4059 stars. I saw something of the preparation of this Catalogue and I know what the achievement at the Cape means.

It means more than incessant work on the part of the Director himself; it means that he had the power of inspiring faithful and ungrudging labour on the part of his assistants. Since I have been in Oxford I have heard the testimony of those who have worked under him at the Radcliffe Observatory; and I can better understand their enthusiastic following where he led. It is, I hope, not any violation of confidence to try and represent in words a side of his character that those who have worked under him or with him will no doubt recognize, however clumsily I may set it down here. I gather that when occasions arose when it was necessary to point out some mistake on the part of one of his assistants—perhaps a mistake implying some carelessness, or having at least that appearance—and when Mr. Stone had spoken in his energetic way, with some sharpness possibly, he would almost immediately leave the room; but in a few minutes he would return and address the offender on some independent topic in the friendliest of tones. His return might be expected all the sooner if the sharpness had been a little exceptional; and, indeed, on many occasions the arrest of his footsteps could be noted before he had reached his own room across the hall.

The Cape Catalogue left him little time for other work during the years 1870–79, though his observations of the Total Solar Eclipse of 1874 were excellent. In his time at Oxford he has produced a similar Catalogue of more northern stars (the Radcliffe Catalogue of 6424 stars for 1890), but he could here work more leisurely and had time for other projects. He undertook the preparation of the observers for the Transit of Venus in 1882, and the discussion of the observations—a considerable labour. When Dr. Gill wanted heliometer observations of the minor planets (Iris, Victoria, and Sappho) Mr. Stone sat up till all hours of the night (though then nearly 60 years old) trying to secure "complete sets" in the variable Oxford climate. Last year he returned to eclipse work after an interval of a quarter of a century, and came back from Nova Zembla, after enduring cold and hardship and narrowly escaping shipwreck, one of the few successful observers, and impressed with the accuracy of his own observations of 1874. On the day before his death he was selecting a site for setting up his instruments for trial preliminary to the expedition to India next January.

His death was very sudden. The week before he had been on a fishing expedition and had had a slight accident. He was devoted to fly-fishing, which he said required all his thoughts, and so took his attention completely away from his work; and he was remarkably successful with both rod and gun. His accident may or may not have contributed to the sudden end on Sunday morning; but the doctor who saw him on that morning gave no such idea, or any warning that he was in any danger. He leaves a wife, a son, and three daughters.

Mr. Stone was, in turn, Secretary and President of the Royal Astronomical Society, and a Fellow of the Royal Society. He received the Gold Medal of the former, as above stated; and the Lalande Prize of the French Academy in 1881. He also received the degree of Doctor of Science from the University of Padua.

H. H. TURNER.

The Great Meteoric Shower of November.

[Continued from p. 203.]

HAVING already referred to some of the principal features in the history of the Leonids, it will be desirable to consider some of the details of observation in connection with this stream. And here I would take occasion to mention that the minutiæ or apparently insignificant features of a shower always deserve critical attention. Details, though frequently ignored as unworthy of record, and perhaps almost obliterated by more imposing features, are often indicative of most important consequences when properly investigated and understood. It is not too much to say that the more brilliant aspects of a meteoric shower are not necessarily the most significant, though usually monopolizing chief attention. In further pursuing the subject, it is intended to refer to the following points:—

The position of the apparent astronomical radiant of the Leonids.
The character of the radiant, its duration, area, and possible mobility.
The visible aspect of the individual Leonids.
Their real paths in our atmosphere.
The minor showers visible at the Leonid epoch.
The expected phenomena at the ensuing return of the Leonids.
The observations necessary.
The November shower from Biela's comet.

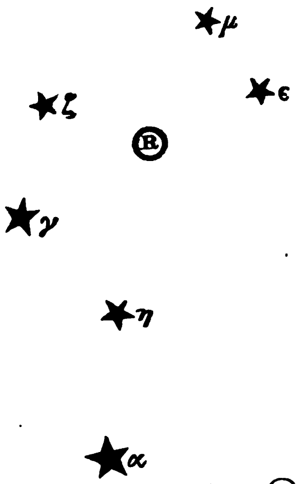
Position of the Radiant.

No observation of a meteoric display can be regarded as complete unless it includes a careful determination of the co-ordinates of the centre from whence the meteors radiate. This is usually termed the "radiant-point;" but it must confessed that the expression is liable to convey a mistaken idea, since the radiant is not a point, but often an area several degrees in diameter. It must therefore be understood to mean the centre of the particular region from which the meteors emanate. As regards the Leonids, the radiant appears to have been first definitely ascertained by Prof. Aiken at Emmettsburg, U.S.A., and Prof. Olmsted of Yale College, U.S.A., in 1833, and it has been subsequently determined by a host of observers, the most accordant positions being as follow:—

Radiant-point of the Leonids.

Year.	Date.	Radiant. α. °	′	δ. °	′	Observer and place.
1833 ...	Nov. 12......	148	10	+23	45	Aiken, Emmettsburg, U.S.A.
„ ...	12......	150	0	+20	0	D. Olmsted, Yale.
1836 ...	12......	150	0	+20	0	G. O. S., New York.
1865 ...	12......	148	0	+23	0	A. S. Herschel, Hawkhurst.
„ ...	12......	148	0	+23	0	H. A. Newton, New Haven.
„ ...	12......	148	0	+24	0	B. V. Marsh, Philadelphia, U.S.A.
„ ...	12......	148	0	+24	0	E. Heis, Munster.
1866 ...	13......	147	17	+23	15	E. J. Lowe, Nottingham.
„ ...	13......	150	40	+22	45	G. F. Burder, Bristol.
„ ...	13......	151	30	+23	0	J. Graham, Darlington.
„ ...	13......	147	0	+24	0	T. Crumplen, London.
„ ...	13......	148	9	+23	48	J. F. W. Herschel, Hawkhurst.
„ ...	13......	149	0	+24	0	A. S. Herschel, Glasgow.
„ ...	13......	148	50	+23	0	Observers at Greenwich.
„ ...	13......	147	35	+22	53	R. Grant, Glasgow.
„ ...	13......	150	30	+21	36	J. Plummer, Glasgow.
„ ...	13......	148	57	+24	18	H. Macleod, London.
„ ...	13......	148	44	+22	33	F. C. Penrose, Wimbledon.
„ ...	13......	148	17	+23	18	C. Pritchard, Isle of Wight.
„ ...	13......	148	0	+25	0	W. H. Wood, Birmingham.
„ ...	13......	149	12	+22	37	D. Smith, Birmingham.
„ ...	13......	149	30	+23	0	C. M. Goulier, Metz, Germany.
„ ...	13......	152	42	+22	19	S. H. Miller, Wisbeach.
„ ...	13......	152	18	+24	6	R. P. Greg, Manchester.
„ ...	13......	149	33	+22	57	J. Baxendell, Manchester.
„ ...	13......	149	8	+23	15	T. W. Backhouse, Sunderland.
„ ...	13......	148	0	+22	0	W. R. Dawes, Haddenham.
„ ...	13......	148	0	+24	0	J. R. Hind, London.
„ ...	13......	150	57	+20	32	J. Birmingham, Tuam.
„ ...	13......	149	0	+22	0	J. C. Adams, Cambridge.
„ ...	13......	150	26	+21	41	F. Brunnow, Dublin.
„ ...	13......	149	39	+23	12	J. Challis, Cambridge.
„ ...	13......	149	0	+24	0	J. H. Gladstone, Nottingham.
„ ...	13......	147	30	+23	15	H. A. Newton, New Haven.
„ ...	13......	147	30	+24	30	A. C. Twining, New Haven.
1867 ...	13......	150	15	+22	31	G. Sands, Washington.
„ ...	13......	147	45	+23	0	F. Bradley, Evanstown, U.S.A.
„ ...	13......	150	45	+21	55	J. C. Watson, Ann Arbor, U.S.A.
„ ...	13......	148	0	+23	0	W. Harkness, Richmond, U.S.A.
1868 ...	13......	152	0	+18	0	B. J. Gillman, Williamstown, U.S.A.

Year.	Date.	Radiant. α. °	′	δ. °	′	Observer and place.
1869 ...	Nov. 13......	151	0	+21	30	G. L. Tupman, Port Saïd.
1876 ...	19–20.	149	0	+22	0	W. F. Denning, Bristol.
1877 ...	13......	146	0	+26	0	T. W. Backhouse, Sunderland.
„ ...	10–13.	148	0	+24	0	W. F. Denning, Bristol.
1879 ...	13–15.	147	0	+23	0	S. J. Perry, Stonyhurst.
„ ...	11–13.	148	0	+25	0	H. Corder, Chelmsford.
„ ...	13......	148	0	+23	0	W. F. Denning, Bristol.
„ ...	15......	151	0	+22	0	E. F. Sawyer, Cambridge, Mass.
1880 ...	11–12.	148	0	+22	0	E. F. Sawyer, Cambridge, Mass.
1885 ...	14–17.	150	0	+22	0	W. F. Denning, Bristol.
1887 ...	14......	155	0	+25	0	D. Booth, Leeds.
„ ...	14......	150	0	+22	0	W. F. Denning, Bristol.
1888 ...	13......	149	0	+22	0	W. F. Denning, Bristol.
1890 ...	13–14.	151	0	+24	0	T. W. Backhouse, Sunderland.
1892 ...	19......	153	0	+21	30	H. Corder, Bridgwater.
1895 ...	13......	151	0	+23	0	A. S. Herschel, Slough.
„ ...	13–17.	154	0	+23	0	H. Corder, Bridgwater.
„ ...	13......	154	0	+24	0	D. Booth, Leeds.
„ ...	13......	150	0	+23	30	E. R. Blakeley, Dewsbury.
1896 ...	14......	149	0	+22	0	A. S. Herschel, Slough.
„ ...	13–14.	148	0	+24	0	H. Corder, Bridgwater.
„ ...	13......	150	0	+24	0	E. R. Blakeley, Dewsbury.
„ ...	14......	150	0	+22	30	W. F. Denning, Bristol.
„ ...	14......	149	0	+23	0	W. H. Milligan, Belfast.
„ ...	14......	152	0	+21	0	J. W. Mears, Calcutta.
	10–14.	148	0	+22	0	J. F. J. Schmidt, Athens.
	7–15.	153	0	+22	0	Greg and Herschel (1863).
	12–13.	148	0	+24	0	E. Heis, Munster.
	11–15.	149	0	+23	0	R. P. Greg (1876).
	1–15.	147	0	+22	0	W. F. Denning (from various observations).



Position of the Radiant Ⓡ amongst the stars in the Sickle of Leo.

The mean position deduced from these (70) values is

$$149^\circ\ 28'\ +22^\circ\ 52'.$$

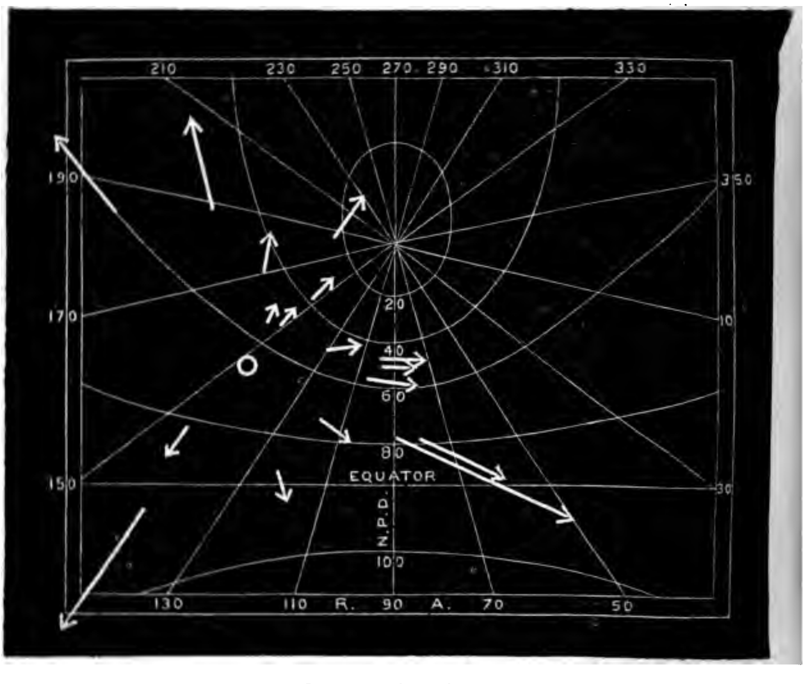
There is a star (mag. 5·7) just visible to the naked eye at this place; it is *x* Leonis of Bode, and corresponds to Piazzi IX. 230, the place for 1890 being

$$149^\circ\ 10'\ +22^\circ\ 28'.$$

The general agreement of the positions included in the table is very satisfactory, considering the nature of the observations, and that some of the observers who witnessed the shower in 1866 had little experience in meteoric work. The Leonid radiant is, however, comparatively easy to find, as it lies amid the bright and well-known stars of the sickle, which serve as excellent guides, and moreover, the streaks evolved from nearly all the brighter meteors of this system afford the means of recording their directions of flight with considerable accuracy.

Prof. Adams, in his historical paper on the orbit of the November meteors ('Monthly Notices,' xxvii. and lvii.), adopted the radiant at 149° 12′ +23° 1′ from 6 values, and this agrees within 20′ with the average place above given from 70 values. A few wisely-selected positions will, no doubt, give as reliable a centre as a large number of miscellaneous observations.

Different observers employ different methods of fixing the radiant. Some prefer a celestial globe, on which to mark with a soft pencil the tracks as observed. Others use star-charts specially prepared for the purpose, such as the maps formerly published under the auspices of the Luminous Meteor Committee of the British Association. Others, again, utilize an ordinary star-atlas, which is, however, only serviceable for registering paths close to the radiant. Another class find it sufficient during an abundant shower to merely estimate the position of the radiant. This can



Paths of 17 Leonids observed at Bristol on the morning of 1879, Nov. 14.

be easily done from the stationary and short-pathed meteors appearing at or close to the centre, but it is scarcely so satisfactory as carefully recording the paths, and deriving the focus of divergence by carrying back the lines of flight of a large number of meteors. The eye and memory, if quite unsupported in the effort to mark and retain positions, are liable to become fallacious guides, so that, in order to ensure accuracy, it is advisable to depend upon actual records.

It does not appear probable that further observations of the radiant will lead to its more precise determination. So many practised observers have applied themselves to the derivation of this feature that further attempts in the same direction will only multiply our data without necessarily increasing the very important element of accuracy. The ordinary eye-method of recording meteors is at best but an approximate one, and if we require greater precision we must apply another agency to obtain it. The capabilities of photography have been invitingly suggested for this purpose, and attempts have been recently made to utilize it, but unfortunately no success has awaited upon the initial efforts. Further endeavours in the same direction may perhaps be more satisfactory, and it may be depended upon that further experiments will be made to render this means effective.

Duration of the Shower.

The extremely feeble character of the display at its commencement and termination has prevented our determining its exact visible duration. True Leonids are undoubtedly to be seen between about November 7 and 20, and quite possibly they may begin to fall on November 5 or 6, but the evidence is not conclusive on the point. A search through catalogues of shooting stars and amongst descriptions of the Leonid shower gives some evidence of its activity as early as November 7 and 8; thus Mr. Backhouse, at Sunderland, in referring to the return in 1871, says he was surprised to see two Leonids on Nov. 8. One of these appeared at 10^h 37^m; it was as bright as Sirius, and perfectly similar to the Leonids in all respects, but its path was not quite conformable with the usual position of the radiant. A 2nd mag. meteor was also seen on Nov. 18, agreeing in appearance and path with those of the Leonid shower. Lieut.-Col. Tupman, in his 'Observations of Shooting-Stars,' ascribes 11, and probably 13, meteors among 32 registered on the night of 1869 Nov. 9 as belonging to his radiant of the Leonids, No. 100 in his list. But a careful examination of the paths shows that, except in two or three cases, they were not divergent from the correct position within the "sickle," and that they belonged therefore to other showers in the same region. One of the undoubted Leonids recorded by Lieut.-Col. Tupman on the date mentioned was as bright as Jupiter; it appeared at 13^h 37^m G.M.T., moved very rapidly along a path 70° +13° to 55° +7°, and left a streak of 6° for 1 sec. In 1877, on Nov. 7, the writer at Bristol saw several Leonids among 81 meteors observed during a watch maintained during the 9 hours from 9^h to 18^h. One of these, a fine 1st mag., appeared at 14^h 32^m, and moved swiftly along a course of 18°, from 110° +7° to 95° −2°, leaving a bright streak. On Nov. 10 in the same year a few other Leonids were recorded at Bristol during a watch from 12^h to 14^h.

Further observations will probably corroborate the evidence here adduced that the shower opens on November 7. Unlike the Perseids of August, which terminate abruptly a few days after the maximum, the Leonids appear to linger for a longer period than that covered by their increase. In 1876 November 20 the writer registered several unmistakable Leonids near the radiant. In 1892 November 19 Mr. Corder saw 4 Leonids which indicated a radiant at 153° +21½°. In 1895 November 17 Mr. Corder also observed 8 Leonids. In effect, therefore, the visible activity of the shower extends over 14 nights. The place of the radiant has not been determined earlier than November 10, owing to the paucity of meteors, but it might be deduced by combining various results.

Character of the Radiant.

Many observers in describing their results refer to the radiant. Prof. Herschel found that 60 of the apparent paths recorded by Mr. Glaisher's staff of observers at Greenwich on 1866 November 13 passed a small circular area 10° in diameter, with its centre at the star *x* Leonis (B. A. C. 3423).

Mr. F. C. Penrose, at Wimbledon, who witnessed the same display, concluded that a circle of 3° appeared to be more consistent than a point for the radiant.

M. Goulier, of Metz, stated that the radiant was so sharply defined that the uncertainty attaching to the position he gave (149° 30′ +23°) was less than 1°. His supposition has proved perfectly correct, for his radiant differs less than ¼° from the mean of 70 determinations quoted on a previous page.

In 1868 November Prof. Newton considered that the radiant would be best represented by a short line extending between the stars ε, γ Leonis, from about the star *x* in the centre of the Sickle to γ Leonis. "The direction of the elongation of the radiant-region is towards the Sun's apparent place, a conclusion which is regarded by Prof. Newton as throwing light of some importance upon the theory of the November meteor-stream."

A large number of additional references might be selected bearing upon the character of the radiant. They are, however, frequently so diverse in nature that they can hardly be said to prove anything. Some observers speak of the radiant as extremely diffuse and extending right over the space lying within the curve of the Sickle, while certain others mention it as a very sharp and definite focus in, or nearly in, the centre of the curve. Many agree that its position is readily and accurately determinable from the stationary or foreshortened meteors appearing in or near it. The writer has usually found the radiant a very well-marked and contracted centre, in fact the best observed Leonids, when their lines of flight are traced back, usually intersect almost at a point. A small proportion of the meteors seem to be a little erratic, but this may be due to mistakes in identifying the true Leonids. The Leonid radiant may be certainly regarded as a very limited focus and entirely different in character to that of the Andromede meteor-shower in 1885 November 27, the radiant of which formed a large area of 7° or 10° diameter.

The observations required to definitely ascertain the particular form of a meteoric radiant must necessarily be of a very critical and accurate kind, as well as pretty numerous. Great care must also be exercised in attributing the meteors observed to their proper radiants. The members of other streams in contemporary action tend to complicate matters and to impart an appearance of diffusion to the main stream unless they are suitably allowed for. A thorough knowledge of the various showers is therefore necessary in a research of this kind. Undoubtedly some meteoric radiants are very scattered and indefinite, but many varieties exist, and the feature named is often only an apparent one introduced by observational errors and mistakes in allocation. In the mornings of November meteoric radiants are so closely packed in the south-eastern sky that great experience and discrimination are necessary both in gathering and reducing materials. This is an enquiry in which photography may afford us valuable information if it can be successfully applied. A photograph of the region of the sky at and surrounding the radiant of a rich shower would, if a sufficient number of trails were found impressed on the plate, go far to settle the feature under discussion.

Is the Radiant-point fixed or movable?

This question was discussed in the 'Observatory,' 1897 Jan., p. 55, and little need be added to the particulars there given. Though the feature alluded to might be disposed of by close observation on a series of clear moonless nights, it does not appear that any adequate effort has hitherto been made to investigate it. According to the late Dr. Kleiber, the radiant should become displaced 1° daily to the eastward, but the positions observed by the writer between Nov. 10 and 20 show no such movement. The data are, however, quite insufficient to test the matter definitely, and it is hoped that attention will be paid to this circumstance during the next four years. It may be confidently expected that meteors will be plentiful enough during the 4 or 5 nights preceding and following Nov. 14 to allow the radiant to be separately determined on each date. Long watches may, however, be essential on the opening and closing nights of the display, as it is sure to be weak at such times, and it is desirable to ascertain the place of the radiant on dates as widely asunder as possible, as they are more likely to exhibit a displacement than those for succeeding nights.

W. F. Denning.

[To be continued.]

Nebulæ, unrecorded in the Catalogues, found on Photographs taken at Starfield, Crowborough.

The photographs here referred to were taken with the 20-inch reflector at my observatory on Crowborough Hill on the dates affixed to each respectively; and the catalogues referred to in the heading are the 'New General Catalogue' and the 'Index Catalogue' by Dr. Dreyer, published in the 'Memoirs of the Royal Astronomical Society.' I wish to express my indebtedness to Dr. Dreyer for collecting the materials from all available sources and compiling, with manifest labour and care, this book of reference so valuable to astronomers.

The co-ordinates of the positions of the new nebulæ are given from measurements made from the nucleus of the nearest object to them found recorded in the N. G. C., the epoch of which is the year 1860.

Region of ♅ I. 217 Aurigæ, N. G. C. 1579; Nova, R.A. 4^h 21^m 38^s, N.P.D. 54° 51'·9.—Nebulosity very faint; circular in form; 15th mag. star involved on the north margin and 17th mag. star close to the north preceding side. 1894 January 9th.

Region of ♅ II. 37 Virginis, N. G. C. 4527; Nova, R.A. 12^h 25^m 28^s, N.P.D. 86° 34'·4.—Pretty faint; elongated north following to south preceding; no structure or nucleus; 17th mag. star on the north preceding side, and a bright star on the south following side. 1894, March 25th.

Region of ♅ I. 92 Comæ Berenicis, N. G. C. 4559; Nova, R.A. 12^h 30^m 23^s, N.P.D. 61° 1'·2.—Like a 15th mag. star involved and surrounded by faint nebulosity; a 15th mag. star 102" north following and a 14th mag. near it. 1894, May 1st.

Nova R.A. 12^h 32^m 21^s, N.P.D. 61° 18'·6.—Very faint; elongated south following to north preceding and pointing to a 15th mag. star 163" distance from the south following end. Within a radius of 10', with this object as a centre, are several nebulous stars of 16th to 17th mag. 1894, May 1st.

Region of ♅ VI. 41 Draconis, N. G. C. 6412; Nova, R.A. 17^h 26^m 21^s, N.P.D. 14° 1'·2.—Pretty bright; elongated north to south; no distinct nucleus; bright star south preceding and 15th mag. star north following. ♅ VI. 41 is described as a globular cluster, but it is a left-hand spiral nebula with stellar nucleus and a small bright star and five or six stellar condensations in the spirals. 1895, September 11th.

Region of ♅ VIII. 76 Cygni, N. G. C. 6991; Nova, R.A. $20^h\ 51^m\ 11^s$, N.P.D. 43° 7'.—The nebula precedes and involves the star D.M. 3011, Zone +46°, mag. 6; is about 6½' in length from north to south, and 5' in breadth from preceding to following; irregular in structure and in outline; nebulosity of a fleecy character, with many stars, both bright and faint, involved in it. 1895, September 13th.

Region of ♅ II. 240 Pegasi, N. G. C. 7814; Nova, R.A. $23^h\ 54^m\ 57^s$, N.P.D. 74° 12'·4.—Very faint; round, about 30" in diameter; no structure. 1895, November 17th.

Nova, R.A. $23^h\ 55^m\ 28^s$, N.P.D. 74° 9'·4.—Faint; elongated north to south; one of three 15th mag. stars involved; condensation at the northern extremity; length, including the star 42". 1895, November 17th.

Nova, R.A. $23^h\ 55^m\ 31^s$, N.P.D. 74° 11'·8.—Bright; small; slightly elongated preceding to following; faint nucleus; 17th mag. star close to the preceding end. 1895, November 17th.

Nova, R.A. $23^h\ 56^m\ 2^s$, N.P.D. 74° 49'·8.—Bright; elongated north following to south preceding about 50"; stellar nucleus; about 17th mag., four 12 to 16 mag. stars on the south near it. 1895, November 17th.

Region of ♅ I. 157 Trianguli, N. G. C. 672; Nova, R.A. $1^h\ 39^m\ 39^s$, N.P.D. 63° 22'·3.—It is nearly as large and prominent as ♅ I. 157, and distant from centre to centre 8' only; nucleus consists of 6 faint stellar condensations forming a straight line in the direction south following to north preceding, and there are 6 or 7 very faint condensations of nebulosity near the preceding margin; 15th mag. star on the north preceding margin and a 16th mag. star at the south following end of the nucleus. 1896, November 29th.

It is remarkable that this object should have escaped detection by the many keen observers who have examined the nebula ♅ I. 157, which is only four minutes of arc distance from it; and it appears to me that we are justified by the evidence in inferring that this nebula has come into the state of visibility during the past half-century. Lord Rosse in 1876 made seven observations of the nebula adjoining, but does not refer to this one. It is remarkable also that the nuclei of the two nebulæ are straight lines of faint nebulous stars immersed in nebulosity, and they are so clearly depicted on the photograph that I think they should be visible to the eye by aid of telescopic power.

Nova, R.A. $1^h\ 42^m\ 19^s$, N.P.D. 63° 29'·7.—Faint; elongated south following to north preceding; faint stellar nucleus; probably a spiral; cluster of stars 9 to 15 mag. on the south side 13" following the nucleus. 1896, November 29th.

I find that the nebula Swift IX. No. 165 'Index Catalogue' is the same object as ♅ II. 612, N. G. C. No. 684.

Region of M 87, 89, and 90 Virginis; Nova, R.A. $12^h\ 26^m\ 7^s$, N.P.D. 76° 22'·0.—Faint; small; round; no structure. 1892, April 29th.

Nova, R.A. $12^h\ 26^m\ 13^s$, N.P.D. 76° 22′·3.—Probably a small spiral nebula with a nucleus like a 16th mag. star surrounded by nebulosity. 1892, April 29th.

Nova, R.A. $12^h\ 26^m\ 44^s$, N.P.D. 75° 54′·0.—Faint; round; small; with a nucleus like a 16th mag. star. 1892, April 29th.

Nova, R.A. $12^h\ 28^m\ 22^s$, N.P.D. 76° 28′·5.—Small; round; some nebulous condensations involved. 1892, April 29th.

Nova, R.A. $12^h\ 29^m\ 40^s$, N.P.D. 75° 58′·5.—A streak of nebulosity extending in north preceding direction from a 13th mag. star; two 12th mag. stars near, and the faint *comes* of the one on the north following side seems to touch the nebulosity. 1892, April 29th.

It is necessary to distinguish on photographs between *faint* stars and *small* stars, for I find on numerous plates, taken during the past eleven years, stars which have a small bright nucleus surrounded by nebulosity, and they are quite distinct from the normal faint stars which appear as small round spots without a nucleus. If I were to register all those faint stars having a small bright nucleus, the list would be a very long one, and the objects classified as "small circular nebulæ with small bright stellar nuclei."

ISAAC ROBERTS.

Starfield, Crowborough, Sussex,
1897, May 1st.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Annular Eclipse of March 15, 1858.

GENTLEMEN,—

It is now nearly forty years since an eclipse of the Sun was central over any part of England, the last being that of 1858 March 15, in which the shadow passed in a north-easterly direction from the western part of the coast of Dorsetshire, through Wiltshire, Oxfordshire, and Northamptonshire to the Wash. It was annular, but probably momentarily total over part of the Atlantic Ocean near the island of Madeira, in consequence of the augmentation of the Moon's diameter. Those of us who are old enough will remember the extensive preparations made to observe it in this country along the line of centrality, which were nearly all unsuccessful on account of the unfavourable state of the weather.

About two years ago was published the 'Life and Letters of Dean Church,' edited by his daughter, Miss Mary C. Church. He was at the time of the Eclipse rector of the small village of Whatley, about three miles from Frome in Somersetshire, which was on the line of centrality, and describes what he saw in a letter to Mr. Johnson, the Radcliffe Observer. The Sun was seen through cloud at the beginning of the Eclipse, afterwards only for a moment about $12^h\ 30^m$, when about three-fourths covered, and again about $1^h\ 2^m$, when the annulus had become a thin crescent, a quarter of the complete ring being wanting. There was, therefore, he says, no opportunity of seeing any of the sights indicated as to be looked for: "of observing the change of colour in the sky, for no sky was visible during the whole time, the Sun being seen only through thin clouds; or of noting the effect in bringing the horizon near, for there was a thick mist apparently all round the distance, and slightly even near. After all my lecturing to my school-children, and of Mr. Hind, I am afraid they must have thought me a humbug; for, though the effect was striking, it was not more so than the closing in of evening, except just in that rapid darkening which came on for a minute and then went off again." Some one in the neighbouring country town (presumably Frome) seems to have indulged in a particularly silly lark; for Church, in a letter to his friend Dr. Asa Gray, the great American botanist, writes that this person "sent the common crier round to announce that, in consequence of the disappointment, the eclipse would be repeated next day. I don't know what effect the announcement had; I only know that the bellman took the fee, and very solemnly went round the town to cry the intelligence." Probably the object was to make merry over those who were ignorant enough to be taken in; but it was surely very reprehensible. In a note to the letter, inserted editorially, we are told that the eclipse was total; but this, of course, was a mistake on the part of Miss Church.

Johnson, it will be remembered, died on the last day of February in the following year. Writing to Dr. Gray a few months afterwards, Church says that he had just seen his handwriting at Oxford "in a letter to the widow of my dear friend Manuel Johnson, of the Radcliffe Observatory. I was very much gratified at seeing how much he was appreciated among you; he did indeed deserve it. A nobler mind, a larger heart, I never knew." The remark makes me think of Johnson's successor, Rev. R. Main, whose death took place nineteen years ago next Sunday, the 9th inst.

Yours faithfully,

Blackheath, 1897, May 6. W. T. LYNN.

P.S. (May 17).—Since the above was written, we have all been deeply concerned to hear of the death of Main's successor, Mr. Stone, on the anniversary of that of his predecessor.—W.T.L.

Solar Motion and Stellar Distances.

GENTLEMEN,—

In your May number you publish an appreciative notice of a paper of mine on the Solar Motion as a gauge of Stellar Distances. I think it is only just to state that, shortly after the publication of this paper, I found that its main points had been anticipated by researches of Professor J. C. Kapteyn, which were published by a scientific society in Holland, in 1893. So far as I am aware, no notice of these researches has appeared in the current scientific journals, except that an English abstract by Miss Clerke was published in 1893 in 'Knowledge.' This fact I overlooked.

I now find that in addition to anticipating the principal points of my paper, the future one as well as that actually published, he has gone yet farther by showing relations between the spectra of the stars and their parallactic motion and probable distance. Under these circumstances it seems proper that I should leave the subject in his hands, and limit my second paper to some methods of estimation not hitherto applied.

Very respectfully,
S. Newcomb.

Nautical Almanac Office,
U.S. Naval Observatory,
Washington, 1897, May 17.

A peculiar Spot on Jupiter.

Gentlemen,—

There are spots on Jupiter which remain for several years apparently unchanged, or at least almost immovable. Such are the "great red spot" and the two "garnet-red spots" which I observed from September 20 and 27, 1894, till June 8, 1896.

I have good reason to suppose that there is a brilliant double-spot in the South Temperate Band, above the left "shoulder," apparently in connection with the "great red spot," because every drawing shows it near the same place. Examining my drawings made at the apparition 1894–95 I found three spots above the left "shoulder," which occupy the following positions:—

1895, January	24	λ=352°;	6°;	20°.
	27	λ= 2 ;	10 ;	21
March	16	λ=346 ;	357 ;	10
	18	λ=350 ;	2	
April	4	λ=350		

A glimpse on my drawings published in No. 3322 of the Astr. Nachr. (also in Nos. 1553, 1556, 1560, 1564, 1569, and 1574 of the 'English Mechanic') will show that the identity of these three, a triangle forming spots, is beyond doubt.

My drawings at the apparition 1895–96 (published under the title "Jupiter-Beobachtungen 1895–96 an der Manora-Sternwarte," in vol. lxiv. of the 'Denkschriften' of the Imperial Academy of Sciences, Vienna—8 plates, with 15 charts and 15 drawings, all show that on 16 days between 1895 Nov. 16 and 1896 May 18 spots were seen in positions not very far from those given above, one, two, or three at a time.

During the present apparition I found similar spots in the following longitudes:—

1897, February	24	$\lambda = 342^\circ$;	359°
March	8	$\lambda = 324$;	356
	21	$\lambda = [342]$	
April	2	$\lambda = [334]$;	[8]
	30	$\lambda = [328]$;	355
May	3	$\lambda = [316]$;	[353]

The longitudes in brackets signify that they were determined by micrometrical measurement. Many of the other spots were determined by the time of their transit, others by the mean of their position in various drawings on the same day. Nevertheless allowance must be made for wrong appreciation, so that the numbers given are partially only *approximate* ones. Notwithstanding, the fact cannot be denied that there is a brilliant double-spot to be found above the left "shoulder," whose position does not sensibly change in 2½ years. In order to settle the question whether this spot really has a certain connection with the near "great red spot," it would be advisable that observers of Jupiter should examine their drawings of the last few years and publish the dates and longitudes of similar spots.

I am, Gentlemen,
Yours faithfully,
Leo Brenner.

Manora Observatory,
Lussinpiccolo, 1897, May 16.

Who is a "Discoverer"?

Gentlemen,—

In your review of the first volume of the 'Annals' of the Paris Observatory of Physical Astronomy at Meudon, on p. 212, you repeat what has by this time become—in legal phrase—"a common form," from having been regularly copied from book to book for the last 20 or 30 years. I refer to the words: "M. Janssen's great discovery with Professor Lockyer at the eclipse of 1868." Now this eclipse happened on August 17-18 of that year, and anyone who will refer to p. 88 of volume xxviii. of the R.A.S. 'Monthly Notices,' published in the preceding February, will find a detailed and explicit description of the principle on which the bright lines in the spectrum of the prominences might be seen, devised by Dr. Huggins two years previously. If, then, MM. Janssen and Lockyer were the *discoverers* of this mode of viewing the prominence spectrum, then undoubtedly Dr. Galle discovered Neptune on September 23, 1846; and it is idle to mention either Adams or Le Verrier as deserving any credit in the matter.

Faithfully yours,
William Noble.

Forest Lodge, Maresfield, Uckfield,
1897, May 11.

OBSERVATORIES.

The Flower Observatory, Pennsylvania.—On the afternoon of May 12th, the function took place which marked the practical completion of the above-named observatory, which is an offshoot of the University of Pennsylvania at Philadelphia. Regular observations have been in progress, however, since September 1896. A formal address was delivered by Professor Simon Newcomb on "The Problems of Astronomy." A platform erected in front of the equatorial building was occupied by the speakers, and in front were assembled about four hundred invited guests. Professor Newcomb was introduced by Provost Harrison of the University, who gave a brief account of the bequest of the founder, Reese Wall Flower, after whom the observatory is named. Short addresses of an informal character followed that of Professor Newcomb, the speakers being Dr. W. L. Warner, Mr. J. A. Brashear, Miss Mary Proctor, Dr. George F. Barker, and the Director of the observatory, Prof. C. L. Doolittle.

A reception at the residence of Prof. Doolittle, under the management of a committee of ladies, closed the occasion, which met with the appreciation of all present.

The instrumental equipment comprises an 18-inch equatorial with spectroscope, a zenith telescope, and a meridian circle, each of 4 inches aperture, with a number of smaller instruments, which will be employed for the instruction of students. All of the optical parts are by Brashear, and the instrumental parts by Warner and Swasey.

A series of observations for variation of latitude has been in progress with the zenith telescope since October 1st, more than 1000 observations having already been made.

The equatorial has also been in use during the winter, mainly for measurement of double stars.

PUBLICATIONS.

The Secular Perturbations of Mercury (Astronomical Journal, vols. xvi., xvii.).—In 1882 Dr. G. W. Hill published a modification of Gauss's method for computing the secular perturbations of one planet by another, as far as the first power of the mass of the disturbing planet. The main feature of the method is that integration as regards the disturbed planet is performed by mechanical quadrature: numerical values only of its eccentric anomaly are involved, and this permits integration with regard to the disturbing planet to be performed with the help of certain auxiliary tables published by Dr. Hill (Astronomical Papers for the American Nautical Almanac Office, vol. i.). Dr. Hill applied his own method to the perturbations of Mercury produced by Venus. Finally, Dr. Hill suggested a modification of his method which rendered fresh tables necessary. These were published by M. Callandreau in 1885 in the 'Annales de l'Observatoire de Paris,' t. xviii.

In 1891 Mr. Innes applied Dr. Hill's modified method to the computation of the action of Mars upon the Earth, and added at the end of his paper a new equation of verification based upon the known theorem that the secular perturbation of the major axis is zero (M. N. R. A. S., vol. lii. p. 80). Unfortunately there are some errors in Mr. Innes's work, and more correct values are given by Mr. Asaph Hall in the 'Astronomical Journal' for 1891, using Gauss's method.

During the last twelve months Mr. Eric Doolittle has published in the 'Astronomical Journal,' nos. 376, 381, 386, 392, and 398, computations of the action on Mercury of the Earth, Mars, Jupiter, Saturn, Uranus, and Neptune. He has returned to Dr. Hill's original method, and has made use of Mr. Innes's equation of verification. In the later papers when the distance of the disturbing planet is large he has adopted a suggestion of Dr. Hill's for finding one root of a certain cubic equation, and has found the other two roots by a method of his own. He has also introduced at an earlier stage some fresh formulæ of verification, and he has in some cases, by dividing the circle into twice as many parts as would have been sufficient, pushed his computations for mechanical quadratures to a quite unnecessary length. Identical results would, as it happened, have been obtained with half the amount of labour; but possibly had the shortened computations alone been performed, the results, though equally accurate, would not have inspired equal confidence, and therefore it is perhaps as well that the full work has been done. P. H. C.

MESURES MICROMÉTRIQUES D'ÉTOILES DOUBLES FAITES À ST. PÉTERSBOURG ET À DOMKINO IN 1895. Prof. S. GLASENAPP.—We are always glad to welcome the double star measures of Prof. S. Glasenapp, who spends his summer vacation at Domkino, where he has a delightful climate. His 7-inch refractor always forms part of his luggage, and as he measured 265 stars we may be sure he succeeded in imbibing as much as possible of the pure night air of Domkino, and was well braced up for his University duties at St. Petersburg. The measures are of neglected wide pairs and a fair proportion of close doubles. This is the fourth series of measures made by Prof. Glasenapp.

NOTES.

COMET NOTES.—Ast. Nach. 3422 contains the following definitive elements of Comet 1895 III. (Brooks), by A. Wassilief:—

$$T = 1895 \text{ Oct. } 21^{d}{\cdot}08988 \text{ Berlin M.T.}$$

$$\left.\begin{array}{lr} \omega \ldots\ldots\ldots & 298^\circ\ 46'\ 8'' \\ \Omega \ldots\ldots\ldots & 83\ \ 5\ \ 1 \\ i \ldots\ldots\ldots & 76\ 14\ 45 \end{array}\right\} 1895{\cdot}0$$

$$q \ldots\ldots\ldots\ 0{\cdot}843034$$

This comet was discovered on 1895, Nov. 21, and 78 observations were obtained, ranging over about a month. The comet was faint and ill-defined, and a difficult object to observe accurately However, the above elements satisfy the observations well, and the orbit does not appear to differ sensibly from a parabola. The supposition of identity with the comet of 1652, which was made at the time in consequence of the resemblance of the elements, would therefore appear to be untenable. A. C. D. C.

MINOR PLANET NOTES.—The following planets have been named (Ast. Nach. 3418):—

Number.	Discoverer.	Date of Discovery.	Name.
348	Charlois.	1892 Nov. 28.	May.
422	Witt.	1896 Oct. 8.	Berolina.

The latter name was unofficially announced several months ago.
A. C. D. C.

OBITUARY.—E. J. STONE. It is with deep regret that we have to announce the death of the Radcliffe Observer, Mr. E. J. Stone, which took place very suddenly on Sunday, May 9. An account of this distinguished astronomer is given in another part of this number.

H. B. CHAMBERLIN.—We regret to announce that Mr. H. B. Chamberlin, late of Denver, Colorado, U.S.A., died on 16th May, 1897. Mr. Chamberlin was an enthusiastic cyclist, and on Sunday, May 16, left his home at Surbiton and rode to Egham. After walking up a hill he mounted his bicycle, and after riding one or two hundred yards he fell from his machine and died almost instantaneously. Medical evidence showed that syncope was the cause of death and that unknown to himself his heart had been for a long while in a weak state.

Mr. Chamberlin was born at Manchester in 1847, and at the age of 7 was taken by his parents to America. During the Civil War he rendered excellent service in the Military Service Corps, for which his previous training with what is now the Western Union well fitted him. Subsequently he entered the drug business, combining a sound knowledge of chemistry with much commercial aptitude. About 1876 he became interested in the Young Men's Christian Association, for which he did much, both in America and England. In 1879 his health broke down and he went to Colorado with but little hope of recovery; but the climate worked wonders for him and he settled in Denver, where he soon became one of the leading citizens of the "Queen City of the West."

Amid much commercial activity he found time for science and philanthropy of no common order. He was a keen worker with the microscope, and mineralogy especially attracted him. Astronomy had long fascinated him, and if his knowledge of the science was not profound he at least provided others with the means of studying it to the best advantage. It was his munificence that presented to Denver University the Chamberlin Observatory, erected at a cost of some £17,000, and equipped with a twenty-inch equatorial, with which Dr. Howe has done much good work.

Since 1890 Mr. Chamberlin has resided chiefly in London, and in 1892 he was elected a Fellow of the Royal Astronomical Society. While here he founded the American Society in London, a task which his social qualities enabled him to accomplish with much success.

He was a great believer in a sound scientific training, and he paid the college expenses of many young men with whom he came in contact. His mineralogical exhibit at the Centennial Exhibition in 1876 received the highest award, and the University of Port Worth, Texas, conferred upon him the degree of Ph.D. On the Friday before the Sunday on which he died he attended the meeting of the R.A.S., and many astronomers will be glad to have had a last chance of meeting one who had done so much for the advancement of the science. He won his way among those who knew him by his singularly kind and sympathetic character. He travelled much and had been to many observatories at home and abroad, upon which he was preparing a lecture for the B. A. A.

We may mourn his premature death, but there is something peculiarly fitting in the manner of it. After the strain and stress of an unusually full life he passed painlessly away quite early on a springtime morning in the quiet of an English country lane.

W. S.

Adam Hilger*. By the death of Adam Hilger, which took place on April 23†, the physical sciences, and especially astronomical physics, have suffered a loss which cannot be immediately made good. Standing in the front rank of practical opticians he did much to promote scientific progress along various lines, his thorough scientific training enabling him to undertake optical work of the highest character.

Born in Darmstadt in 1839 he early showed a marked inclination for the mechanical work in which his father was then engaged. For some years he was a mechanical engineer in Darmstadt, and he afterwards entered Ertel's famous establishment at Munich. He next came to London, but though commanding a good salary found no opportunity of advancing his knowledge and soon left for Paris, where he had the good fortune to find employment with the firm of Lerebours and Secretan. During this engagement he constructed many instruments under the direct supervision of Foucault and became fully acquainted with the theory as well as with the practice of his art. After the war of 1870 he came to

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* From 'Nature,' 1897, May 13.

† We believe that Mr. Hilger died from the effects of a bicycle accident at Brighton.

London with his family. Here he was engaged with Mr. Browning, at first as a simple workman, but afterwards as a foreman. Having completed a five years' contract he commenced business on his own account at Islington. At these well-equipped works he produced the instruments which have brought him a high reputation among physicists and astronomers throughout the world. He was especially skilled in manipulating quartz and Iceland spar for work on the ultra-violet rays, and had lately succeeded in making very perfect chromatic combinations of these materials. The special qualities of the new Jena glasses were also well known to him, and by their uses he produced achromatic lenses of very short focal length, as well as prisms of very high dispersion.

We understand that the business will be, in all probability, continued by Mr. Otto Hilger. A. F.

William Godward.—Announcement is made of the death, which took place on May 19, of Mr. William Godward, F.R.A.S., formerly of the Nautical Almanac Office. Mr. Godward joined the staff of the English ephemeris in 1847, and rose to the position of Chief Assistant, to which he was appointed in 1869. He left the service, having reached the age for retirement prescribed by official rules, in 1890. Mr. Godward made several contributions to the literature of astronomy, which are, as might be expected, of a kind useful to computers. His interpolation tables, published in 1857, have been used in the N. A. Office with much advantage for computing the hourly ephemeris of the Moon for many years. In 1866 he published "Auxiliary Tables for computing an approximate Ephemeris of a Minor Planet," and later he computed the "Elements of the Orbit of the Minor Planet Ceres" (Mon. Not. R. A. S. vol. xxxviii. p. 119), which are used in the 'Nautical Almanac' computations to this day. Mr. Godward was 68 years of age at the time of his death.

Wilson's Theory of Sun-spots.—Statistical evidence bearing on the vexed question whether sun-spots are cavities in the photosphere or not is always welcome. Prof. Ricco, of the Catania Observatory, has published, in Ast. Nach. 3423, some results of this nature from a series of drawings, 17,456 in number, made by him either at Palermo or Catania during eleven years.

Out of 3324 spots thus recorded, Prof. Ricco selected 185, of which the nucleus was perfectly central when the spot was at the centre of the disc, and these he has divided into classes according as the penumbra was, when the spot was near the limb, seen to be larger on the side near or opposite to the edge of the disc. The numbers in each class are as follows:—

Spots of which the penumbra was larger on the side near the limb......	131
" " " " opposite "	18
" " " equally large on both sides	36

The first of these three cases is in accordance with the truth of

Wilson's hypothesis that the sun-spots are cavities. The evidence, therefore, in favour of Wilson, or to the contrary, or indifferently for and against, is in the ratio 7·3 : 1 : 2, which is not greatly different from the similar ratios found by De la Rue, Stewart, and Loewy in their well-known researches, or to the result found by Secchi. Considering now the especially significant regular spots, Prof. Ricco finds 23 in favour of Wilson and only one against.

It will easily be realized that from geometrical consideration of the micrometer measures of the breadth of the penumbra on either side of the nucleus that the depth of a spot can be deduced. From the 23 especially regular spots above mentioned, Prof. Ricco found the average depth of a spot to be 1037 kilometres.

On applying Corrections to Transit Observations.—The Note in our April number on Mr. Seabroke's device for computing the usual transit corrections has brought us a letter from M. F. Oom, of Lisbon, who calls our attention to a graphical method arranged by Prof. Rodrigues, the Director of the Lisbon Observatory, for computing level and azimuth corrections, used in that observatory, which was described in the Lisbon volume relating to the meridian observations of Mars in 1892.

This method in the end is a means of arriving by a scale at the value of the formula $n \tan \delta + m$, to which, as is well known, the corrections for level and azimuth are together equivalent. The geometry given in the Lisbon publication may be shortened as follows:—Draw a circle to represent the meridian, with CE the projection of the equator, CZ the line through the zenith, CH the south horizontal line, all in the plane of the meridian; then draw BEA a tangent to the circle at E, cutting CZ produced in B, and CH produced in A. Then if CS be drawn to represent the direction of any star, cutting AB in M, the ratios of the perpendiculars MF and MG on CZ and CH respectively to the radius of the circle are the azimuth and level factors respectively, or, what is the same thing, the azimuth factor varies as MB, and the level as MA for different positions of S.

It is not difficult to see how by taking a length corresponding to AB as a base line, dividing it proportionally to tangents of declination from the point representing the equator, and drawing perpendiculars to represent the level and azimuth errors at the points corresponding to the declinations where the factors are respectively unity, these perpendiculars will form sides of right-angled triangles with opposite angles respectively at A and B, the height of the hypothenuses of which give the level and azimuth corrections for any declination, and by a slight extension of the diagram the sum of the two can be read off from one scale.

Measures of the Companion to Sirius.—Herr Leo Brenner has sent us the following:—"My last measures of the companion to Sirius were made under favourable atmospheric circumstances, so that I have every reason to believe them to be very reliable. The different measures are as follows:—distance = 3″·746, 3″·590, 3″·809, 3″·902, 3″·809, 3″·689, the mean of which is 3″·7575; position-angle = 189° 8′, 190°, 189° 35′, 188° 30′, 188° 16′, 189° 19′, mean 189° 8′. The time of these observations was March 22, 7^{h}–$8\frac{1}{4}^{\mathrm{h}}$ Mid-European. The power used was 310. The mean of 6 measures made on March 17 gave distance = 3″·606, position-angle = 188° 58′, but the result on March 22 is more reliable. The companion was about 9th magnitude, and an easy object in our 7-inch refractor."

A Method of Observing the Satellites of Mars.—Prof. Schaeberle has published some observations of Phobos and Deimos, made during the opposition of 1896 by means of an ingenious device, which it may be of advantage to some observers to know. The essential point of this is that the measures of the satellites are made at once with respect to the centre of the planet. In the eyepiece-cell a glass plate is fixed, so that its inner surface is nearly in contact with the wires of the micrometer. On this surface a black circular disc is made of a diameter slightly smaller than that of the computed disc of Mars, and the centre of this disc is accurately marked by a small transparent circle, across the centre of which a fine spider-line is stretched, which is put in position normal to the moving wires of the micrometer. The image of the planet can be put exactly concentric with this very readily, as any slight eccentricity is glaringly shown by the appearance of some part of the limb of the planet at the edge of the mask. The thin spider-line is used for measuring the position-angle of the satellite, and the zero of distance measures is given by the micrometer reading when the movable wire passed through the centre of the minute central circle.

Distribution of Stars in Clusters.—The following note was communicated to Ast. Nach. No. 3422 by Prof. E. C. Pickering:—"Prof. Bailey has recently made a count of the stars in the vicinity of several clusters. An enlargement was made of a photograph of the Pleiades taken with the Bruce telescope and having an exposure of six hours. A region 2° square, with η Tauri (Alcyone) in the centre, was divided into 144 smaller squares each 10′ on a side. The stars in each of these squares were then counted. The total number thus found was 3972, an average of 28 in each square. The 42 squares, including the brighter stars in the group, contain 1012 stars, an average of 24 per square. It therefore appears that the total number of stars in the region of the Pleiades is actually less than that in adjacent portions of the sky of equal area, and is much less than the corresponding number in many parts of the Milky Way. The Pleiades must therefore be regarded first as a group consisting of comparatively bright stars; secondly if we omit the bright stars, the number of faint stars will be much less than in the adjacent portions of the sky. This absorption of the faint stars is probably due to the nebulosity surrounding this group. A similar absence of faint stars is noticeable near other diffused nebulæ, for example that surrounding N. G. C. 6726–7. This condition would be explained if we assume that stars have not yet been formed by the condensation of this part of the nebula or that the latter is less distant and slightly opaque.

"A similar count was made of ten regions 6′ square, in the vicinity of η Carinæ. The plate used was taken with the 24-inch Bruce telescope, and had an exposure of four hours. From this count it appears that in a region 5° square the total number of stars was about 250,000, while the number contained on the entire plate exceeded 400,000."

At the Conversazione of the Royal Society, which took place on Wednesday, May 19, the astronomical exhibits were mainly photographic. The Astronomer Royal showed a series of photographs of the Moon, taken with the new 26-inch Thompson Photographic Telescope at Greenwich, and the Paris photographic Atlas of the Moon was also shown. Prof. Lockyer exhibited photographs illustrating the enhanced lines in spectra of the Hotter Stars, about which we had a note in our April number, and also a photograph showing the presence of a fluting of carbon in the ultra-violet spectrum of the Sun. Prof. Lockyer also showed photographs relating to the Eclipse of last August—one, the spectroscopic results obtained at Novaya Zemlya; the other a series of pictures illustrating the preparations and arrangements made at Kio Island, Varanger Fjord.

Two of the Editors of this Magazine wish to temporarily dissociate themselves from their colleague in order to congratulate the writer of the "Oxford Note-Book" on his selection for election to Fellowship of the Royal Society. The three Editors unite in offering congratulations to Dr. Charles Chree, Director of the Kew Observatory, on similar honour.

Prof. Hermann S. Davis has sent us a paper in which he deduces $+0''\cdot360 \pm 0''\cdot0146$ as the relative parallax of 61′ Cygni from the Rutherford photographic plates. The material was obtained from 19 negatives, both measures of distance and angle being made use of.

An announcement by the Radcliffe Trustees will be found on page 3 of the cover of this number.

The American Nautical Almanac Office is now under the superintendence of Prof. W. W. Hendrickson, who has been an officer of the American Navy since 1862. Since 1873 he has filled the post of head of the department of mathematics of the Naval Academy at Annapolis.

From an Oxford Note-Book.

Anything from Prof. Lewis Boss on fundamental astronomy is worth our best attention. In A. J. No. 401 he continues his remarks on Division Corrections of Circles. He concludes that the chief duty of an astronomer is to determine the *systematic* errors of the circle; he may do more harm than good by trying to determine individual errors of particular divisions. This is more or less cheerful reading, for though the necessary labour before a new circle can be used to full advantage is still great, it is a distinct relief to feel that there is a reasonable and proper limit to it, as one may assuredly do after the thorough examination of the point by Prof. Boss. I hope, however, that I shall not mislead anyone by this summary statement. All those concerned will doubtless read carefully the papers themselves.

But is it not time that the question is asked whether some better arrangement than a circle cannot be found? Nothing *absolute* is gained by the circle. The divisions are merely marks *near* the truth, and the errors have all to be determined. Cannot we reduce the labour by in some way using a portion of the circle over and over again? As an illustration, suppose a circular arc of 30°, viewed by reflection from the facets of a dodecagonal prism, firmly attached to the axis of the instrument. As the instrument was gradually turned, different portions of the arc from 0° to 30° would be seen, and then the next face would come into action and the portion 0° would again be seen, and so on, for all twelve faces. Then we should have to determine (A) the angles between the twelve faces, which comes to much the same as the fundamental division of a circle; (B) the errors of the single arc, which comes to much the same as the systematic errors of subdivision, but with this advantage, that whereas in the circle the errors of subdivision are only similar, in our case they are actually identical and do not even need separate treatment. Another method would be to have, not a fixed, but a slowly revolving circle, and take transits of a good many divisions. This would ultimately eliminate all *systematic* errors, for the index would be arbitrary, though it would, of course, increase the probable error.

It may not have occurred to some observers that a great deal of information as to the systematic errors of a circle can be obtained from ordinary readings of microscopes in the course of ordinary observations, provided these are well enough distributed. For simplicity take the most fundamental systematic error—that of eccentricity of the circle and its axis. If the law of error be $a \cos \frac{1}{2}(\theta + \epsilon_1)$, then the differences of the readings of two microscopes 90° apart will follow the law $\sqrt{2}\, a \cos \frac{1}{2}(\theta + \epsilon_2)$, where $\epsilon_2 = \epsilon_1 \pm 45^\circ$, and we can determine a and ϵ_1 with as much accuracy from these differences as from any absolute observations. It looks at first sight, indeed, as though we got an actually bigger coefficient $\sqrt{2}a$ instead of a; but of course the probable error of a difference of two microscopes is greater than that of a single reading.

General Tennant writes:—

> I see you have been talking about sagging of wires: this is not always an imaginary evil, even when the astronomer does his best. I have known wires sag in wet weather badly when they had been put in dry, and the only cure I found was washing them and stretching them: one can sometimes make a wire double the natural length by this process, but they need care in fastening as the strain is considerable as they dry. I do not think that instrument-makers ever take this trouble, at all events I never saw it described in any instructions: all the apparatus one wants is a camel's hair brush full of water and a pair of large spring dividers on which to fix the thread temporarily. Of course one needs a steady hand, but if you are content with moderate elongation (say 50 per cent.) there is little risk of failure.

Alongside this interesting statement I should like to place two extracts from the Astronomer Royal's Reports for 1866 and 1867:—

> In connexion with this, however, I have to mention an investigation lately made by Mr. Stone (First Assistant). Mr. Stone made observations of the meridional zenith-distance of the same object at different points of the same horizontal wire. On comparing these, it appears certain that the middle part of the wire drops, in reference to the parts near the first and last vertical wires, by nearly one-fifth of a second. This, it is evident, enters into the determination of instrumental flexure. It does not affect the validity of the numerical astronomical correction, except to this extent, that, according to the state of tension, there may be a small variation between the corrections on different days. It may be well to take measures for stretching the horizontal wire with the greatest force that it will bear. (1866, June 2.)
>
> The plan of stretching the horizontal wire in the field of view has proved unsuccessful; the wire soon broke, apparently from dampness of atmosphere. (1867, June 1.)

Some time ago a protest was made in these Notes against the use of unnecessary figures—too many decimal places; and the subject was by no means exhausted, though perhaps quite as much was said as could have appropriate place among such miscellaneous jottings as these. I should like now to call attention to the other side, and protest against the importance attached to an existing device for saving labour which goes by the name of "adapting formulæ to logarithmic computation." Perhaps it will shock people to have the efficiency of this antiquated old machine called in question: it has been in the text-books for so long, and we have all learnt it as gospel. But how much does it save? Not more than a fraction of the labour cheerfully wasted in using a decimal place too many, against which the former protest was made.

Let us take an example from the following familiar formula:

$$\cos a = \cos b \cos c + \sin b \sin c \cos A.$$

To "adapt it to logarithmic computation" we put

$$\tan x = \tan b \cos A,$$

and then

$$\cos a = \cos b \cos (c - x) \sec x;$$

and this pair of equations would be considered much more labour-saving to work with than the original one. Now if anyone will kindly go through the operations for finding a in the two cases, they will find them to be as follows:—

As the Equation stands.	*As "adapted."*
8 *references to tables*, viz.:	7 *references to tables*, viz.:
$\log \sin b$, $\log \cos b$.	$\log \tan b$ and $\log \cos b$.
$\log \sin c$, $\log \cos c$.	$\log \cos A$.
$\log \cos A$.	x and $\log \sec x$ from $\log \tan x$.
Nos. for $\log (\cos b \cos c)$ and $\log (\sin b \sin c \cos A)$.	$\log (c-x)$.
a from natural cosine.	a from $\log \cos a$.
Of these 2 pairs are taken out at same opening.	Of these 2 pairs are taken out at same opening.
3 *additions*, viz.:	3 *additions*, viz.:
$\log \cos b + \log \cos c$,	$\log \tan b + \log \cos A$,
$\log \sin b + \log \sin c + \log \cos A$, and	$\log \cos b + \log \cos (c - x) + \log \sec x$, and
$\cos b \cos c + \sin b \sin c \cos A$.	$c - x$.

Thus it will be seen that *out of* 11 *operations we only save one reference to tables*; and in my opinion this is almost counterbalanced by the fact that one of the additions in the second case is the formation of $(c-x)$ involving the subtraction of two angles, which takes more time than that of two logarithms; and, moreover, we introduce the labour of determining which quadrant the new angle x is in—not a very serious labour perhaps, but one in which we may easily make a mistake. I should much like to set a practiced computer to determine a for 100 stars by the two methods and see what gain there was. Perhaps I will do so. Other cases I believe to be generally similar, though I may have overlooked some exceptions. If so I should very much like to hear of them.

ON June 1 a silver cup filled with mint-julep is passed round the Hall of New College, Oxford, and sipped by all those present. The custom is a comparatively modern one and originated as follows:—About 20 or 30 years ago an American visited the College and was good enough to express his satisfaction with the hospitality he had received, though he was shocked at the ignorance of mint-julep which he found there. On leaving he presented the Cup to the College, with a request that it might be filled with mint-julep every first day of June and handed round as a loving-cup; and that a place might be reserved for him on that day, on the chance that he might be able to return. The empty chair has been religiously set for him each year, but he has never returned. [American papers please copy ?]

EDWARD JAMES STONE, F.R.S.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, June 11, 1897.

Dr. A. A. Common, F.R.S., *Vice-President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., F.R.S., and H. F. Newall, M.A.

The Chairman. I am sure you will feel with me that it is a matter for regret that Sir Robert Ball, our President, is not able to take the Chair to-night owing to the fact that he has had to undergo an operation on one of his eyes; but I am happy to say, from the latest information received, that the operation has been successful, and it is likely that at our next Meeting we shall have Sir Robert Ball again with us.

The Minutes of the previous Meeting were read and confirmed.

Mr. Newall. Each time I take up the list of presents there seems to be an unusual number received since the previous Meeting. Eighty-one have been received since we were here in May. Amongst others, I may call attention to a new edition of Prof. Young's book, 'The Sun,' a volume of the International Science Series, revised and improved. We have also received a copy of the tables for computing star-constants by the late Mr. Stone, altered and re-arranged by Prof. Turner. There is also a small pamphlet by Prof. E. C. Pickering, of the Harvard College Observatory, entitled 'Navigation in Fog,' and another contribution from Harvard in the shape of a volume of the Observatory 'Annals,' part i. of volume xxviii., which relates to the spectra of bright stars. We have some micrometrical observations of double stars by Prof. Glasenapp, of St. Petersburg, and a work on stellar photometry by Dr. Müller, of Potsdam, and, lastly, a copy of the Catalogue of Stars made at the Cambridge Observatory for the zone +25° to +30° N. Declination of the Astronomische Gesellschaft Catalogue.

The Chairman. There is a matter which ought to be mentioned to the Fellows of the Society; and that is, that in this historic month, when everyone is commemorating the 60th year of the Queen's reign, it is proposed that this Society should offer a dutiful and loyal address to Her Majesty. The Council, after due consideration, have prepared the following Address, which it is right that I should read to the Meeting:—

> MAY IT PLEASE YOUR MAJESTY,—We, the President, Council, and Fellows of the Royal Astronomical Society, humbly beg to approach Your Majesty with the tender of our sincere and respectful congratulations on the happy completion of the 60th year of Your Majesty's most auspicious reign.
>
> Sixty years ago Your Majesty was graciously pleased to become the Patron of our Society. In approaching Your Majesty on that occasion our Society ventured to express the hope that Your Majesty's reign
>
> > "would be distinguished for the advancement of Astronomy, as well as of every other branch of Science, and for the enjoyment under Divine Providence of every Earthly blessing."
>
> When we see how abundantly these loyal wishes have already been fulfilled, we desire to express our deep appreciation of the interest and sympathy that Your Majesty has always manifested in the intellectual well-being of Your Subjects; and on behalf of all those within the wide limits of Your Majesty's Empire who are interested in the development of Astronomy, we gratefully acknowledge the benefits we have enjoyed during the long continuance of Your Majesty's reign; and pray that for many years Your Majesty may be spared to rule over us.

Prof. Naegamvala read his paper on ♅ I. 43 Virginis, in the course of which he referred to descriptions of the nebula as given by different authorities, and said he hoped to send to the Society a photograph of the nebula at an early date. The nebula was photographically found to be elliptical, which was not indicated in any of the drawings previously made. At the request of the Chairman he then exhibited lantern-slides of photographs taken at Poona:—The nebula of Orion with 65 minutes exposure, taken with 16-inch reflector; the constellation of Orion, taken with an old portrait-lens by Ross, with an exposure of 70 minutes; the cluster Messier 46, taken with 105 minutes exposure, showing the ring nebula; globular cluster Messier 22 in Sagittarius, with exposure of 15 minutes, showing nebulous background; Messier 8, taken with the same portait-lens, with 3 hours exposure, and the same object taken with 1 hour exposure; also M. 67 in Cancer, taken with 16-inch reflector; and the Milky Way in Ophiuchi with a Dallmeyer 1½ portrait-lens, with an exposure of 2 hours.

Dr. Spitta. Have you tried the effect of formalin on your plates?

Prof. Naegamvala. No.

Dr. Spitta. I have made some experiments on the action of formalin, and I find that it prevents the film going to pieces, and that it makes the plate hard and horny. A plate can be fixed with it in an hour, but it should be dried before a fire and not with a spirit-lamp. Formalin is a commercial article.

The Chairman. If you wish to die or to kill your assistant, I should say use formalin.

Dr. Spitta. I have used it for some time and am still alive.

The Chairman. That only shows that you are differently constituted to the rest of the world. I used formalin, and my man had to go home soon after the bottle was opened. He did not know what was the matter with him, nor did I; but I found this formalin had a powerful toxic effect, and I would advise everyone to be very cautious in its use.

Dr. Spitta. I have used it continuously for more than a year, and my son also. Perhaps you used a large quantity?

The Chairman. No. With reference to Prof. Naegamvala's photographs, I should like to say that the particular position that he occupies on this Earth when he is observing is a great advantage to him as compared with the position we occupy. Messier 8 has always seemed a most delightful body to me. I think that in a clear sky it would be a most beautiful nebula and cluster combined. I quite envy Prof. Naegamvala the opportunity he has of photographing the objects he has shown on the screen.

Mr. Newall (reading a paper on "Some Determinations of Velocity in the Line of Sight made at the Cambridge Observatory"). This note gives results recently obtained in determining velocity in the line of sight of some of the bright stars with the Bruce spectroscope at Cambridge. It is a long time since this apparatus has been received, and I am afraid that I was a little premature when I stated in this room nearly 18 months ago that it was in working order, but there have been difficulties in making the preparatory arrangements for this work. The instrument was received two years ago, and it took about six months before I was satisfied that I could begin work. I devoted almost the whole of last year to making determinations of velocity in the line of sight, with discordant results; and the autumn of last year was devoted to ascertaining the troublesome cause of discrepancy between my results and those obtained at Potsdam. I ultimately traced the cause of this difference to its source, and I have now some confidence in saying that the apparatus is in working order. (Mr. Newall then gave an explanation of some details of his work, illustrated by photographs of spectra of stars and the iron lines in the solar spectrum side by side; the star velocities had been determined by micrometric measures of the relative position of the iron lines in the two spectra.)

Prof. Turner. I had very great pleasure in seeing the MS. of Mr. Newall's paper to-day, and I feel sure that, although he has presented it as a more or less ordinary paper, he must be very joyous that after many years of hard work—underground work he has called it—he has at last got his apparatus into order, and that it is turning out results of the very first importance. They are not many in number at present, but by November, or perhaps later, I expect we shall have a larger crop; but I am sure we must congratulate him sincerely on having got this magnificent telescope and spectroscope into such good order, and that he is bringing out such excellent work. As to the agreement of his results with those of Prof. Vogel, when we remember that Prof. Vogel used the hydrogen lines and Mr. Newall the iron lines, I think this independence of method is valuable. The results common to the Cambridge and Potsdam series are not many, but so far as they go they seem to me to be very striking, considering that Mr. Newall has used many independent lines, all belonging to the iron spectrum, and the other results were obtained from an entirely different spectrum, that of hydrogen.

Mr. Seabroke. I must congratulate Mr. Newall upon his remarkable results. Some years ago I dabbled in this branch to some extent. There was shown on the screen a photograph of the spectrum of α Cygni, and by the side of it the spectrum of the Sun, and I would like to ask Mr. Newall how he got these in juxtaposition. I would have given anything at one time to see a solar spectrum by the side of a stellar. Were they taken one in the morning and one in the evening?

Mr. Newall. Yes, that is so. As the scale is necessarily different owing to difference of temperature, this is a troublesome thing to arrange. The original spectra are taken at different times and on different plates, and by means of an enlarging apparatus of special construction the spectra are thrown side by side on a single plate.

Mr. Seabroke. In the photograph which showed an iron spectrum taken before the star spectrum, and another taken after, the iron lines in the spectrum taken earlier seemed to be in almost exact coincidence with the iron lines in the stellar spectrum, but the iron lines in the spectrum taken later did not appear to similarly coincide.

Mr. Newall. It is due to change in temperature, or in the position of the instrument during the taking of the photograph.

Mr. Gregory. I should like to ask if Dr. Vogel always used hydrogen lines. I am under the impression that he sometimes used the iron lines.

Mr. Newall. So far as I know he has only published results deduced from the iron lines in one or two instances, notably in the case of Sirius; but I do not think that he has published such results for any solar stars.

The Chairman. This is rather a technical though a very interesting subject, and it is only those who have done work of this kind who can ask questions of Mr. Newall that are pertinent. Perhaps Mr. Maunder can say something on this subject.

Mr. Maunder. There were one or two questions that I wished to ask, but Mr. Seabroke has already asked them, and I have no further remark to make except to congratulate Mr. Newall on his success.

The Chairman. I am sure all the Fellows present will congratulate Mr. Newall on his success. He has been an ardent worker in astronomy for many years, and we have been looking forward to see the results of his work. I am sure that these are as excellent as we expected them to be, and we will continue to look forward to a more extended series later.

Prof. A. A. Rambaut then read a paper entitled "Notes on the Reduction of Stellar Photographs."

Prof. Turner. I am glad to think that Prof. Rambaut is working at this subject, which has occupied my thoughts a good deal lately from a different point of view. The comparison of formulæ is very interesting.

The Chairman called upon Prof. Naegamvala to say something about the coming Total Solar Eclipse of January next.

Prof. Naegamvala. I did not think that I should be called upon to say anything about the conditions of the next eclipse, which, as you know, will be visible in India. However, the subject is familiar to me now, as I have been thinking about it for more than a year. I am not personally acquainted with the meteorological conditions of the northern part of India; but the distinguished President of the Asiatic Society of Bengal is present here, and he can say more on this particular point than I can. But so far as Southern India is concerned I find everyone proposes to go there: first, because the duration of totality is longest; secondly, because it is cheapest to reach any of the stations there; and, thirdly, because it is nearest for astronomers coming from Europe. I can say something about the conditions there. Last year was a very abnormal year, and we had banks of clouds all over the Deccan in January, but I believe such a thing is sure not to happen next year. I think the chances are 99 to 1 in favour of a clear sky so far as Southern India is concerned. Unfortunately, however, the hour of the day is not the best. From my little experience in observing the Sun, I find that one cannot get a decent view of the Sun after 11 o'clock from Poona, as the air is so very tremulous. The photographic exposures will be, however, of very short duration, and that may go a great way in counteracting the evil effects of an unsteady atmosphere. Targaon and Jeur will be the best places to go to, but observers will have to get all supplies from Poona and must be prepared to camp out. The month is that of January, which is one of the very best months in the Deccan, and camping out will be a pleasant though perhaps a new experience for those who come there. I shall very likely go to Jeur, because Dr. Michie Smith, the Director of the Government of India Observatory at Madras, proposes to go to Targaon. I believe Dr. Smith's plan is to photograph the corona more than to take the spectrum. He is preparing to photograph the corona with a 40-ft. telescope of 6-in. aperture and with the polar siderostat. He also proposes to photograph it with a six-inch refractor, and he is going to use some slit spectroscopes. My plan is more spectroscopic than purely photographic. I propose to take photographs of the spectra of the corona with a 6-in. Taylor-Cooke achromatic lens with 2 objective prisms of 45 degs. angle and 6 × 8 inches face; this will be the largest prism-lens combination that has ever been used on the corona, it will be used with a 12-in. coelostat. Besides this I propose to have a large single prism slit telescope 3 inches in aperture, and to use that on the corona with an 8-in. lens and 12-in. siderostat. I propose to use an objective prism of spar and quartz for the same purpose, 2-in. aperture. I hope to have an integrating-telescope of small size for the spectrum of total radiation from the eclipsed Sun. I hope to have a few photographs of the corona, and also to make ordinary meteorological eye observations. Mr. Maunder the other day described my plans as ambitious, but I devoutly hope that they will not prove too ambitious for me and that I shall be able to obtain some results.

The Chairman. I am sure we have all heard with pleasure the remarks of Prof. Naegamvala on the forthcoming eclipse. I do not think his plans are at all too ambitious and I hope he will be quite successful. He travelled all the way from India to somewhere past the North Cape to get a little experience to do justice to this Indian Eclipse. The little experience he gained then will do something towards helping him to get over that feeling of tremor and anxiety that all eclipse observers must have when they see their first eclipse. We all hope that Prof. Naegamvala will have great success with his various instruments.

Prof. E. W. Brown (having been called upon to read his two papers on the lunar theory). I feel that I should apologize for the technical nature of these papers. It has often occurred to me that among astronomers the lunar theorist has somewhat the attributes of a policeman. You are glad he is there to be consulted when you want to know the way, but at other times most persons prefer to have as little to do with him as possible. However, I will try to give a short account of my first paper and to say a few words on the later history of the lunar theory. There are two great theories of the Moon's motion which have been used during this century for accurate calculation: the theory of Hansen, which was worked out numerically, and that of Delaunay, which was necessarily algebraical. The theory I have been engaged on for the last six years is based on the papers of Dr. G. W. Hill, published in 1877. His work was pioneer work to a large extent, and it is less remarkable for the development which it then received than for its suggestiveness in giving a new method, not only for accurate calculation but also for more theoretical researches. I have been chiefly occupied in developing it for accurate calculation, and in actually making the calculations necessary to find numerical expressions for the Moon's coordinates. The portion now published contains the whole of the theory necessary for this object, and the results of the calculations are given so far as to include all coefficients up to and inclusive of the powers and products of the second degree with respect to the eccentricities, the inclination, and the ratio of the parallaxes. Some results of interest have already been published in the 'Monthly Notices.' These concern the mean motions of the perigee and node and the secular accelerations. Some errors have been detected in the theories of Hansen and Delaunay. One of these, in the motion of the node, which Hansen supposed to be due to a want of coincidence between the centre of mass and centre of figure of the Moon, is probably a mere numerical error. An error in Delaunay's work was, by good fortune, actually traced down. My second paper contains a small correction to the value for the mean motion of the perigee which I gave a short time ago.

Mr. Dyson. I wish to express admiration for Prof. Brown's work. I have only a little acquaintance with the lunar theory, but sufficient to appreciate the immense value of Prof. Brown's work and the very great difficulties which he has surmounted. There is another point which has not been mentioned, but which might be; and that is, that besides developing Dr. Hill's new theory of the Moon's motion, Prof. Brown has given us the best book on the existing theories. On both these grounds I think we may heartily congratulate Prof. Brown.

The Chairman. I am delighted to hear that Mr. Dyson can give his support to the technical side of these papers, and I am sure everyone will agree that such a difficult subject has never been put before this Society so nicely and so clearly as Prof. Brown has put it to-night.

The following papers were announced and partly read:—

Prof. E. W. Brown. "Theory of the Motion of the Moon."

Prof. E. W. Brown. "Note on the Mean Motion of the Lunar Perigee and Node."

C. J. Merfield. "Elements of Comet Perrine (*f*) 1896, November 2."

Prof. K. D. Naegamvala. "Nebula ♅ I. 43 Virginis."

W. F. Denning. "The Shower of Leonids in 1897."

John Tebbutt. "Results of Double Star Measures with the 8-inch Equatorial at Windsor, New South Wales, 1896."

Prof. A. A. Rambaut. "Notes on the Reduction of Stellar Photographs."

Prof. A. A. Rambaut. "Photographic Observations of Comet *b* 1896."

H. F. Newall. "Some Determinations of Velocity in the Line of Sight made at the Cambridge Observatory."

A. Marth. "Ephemeris for Physical Observations of the Moon."

The following gentleman was elected a Fellow of the Society:—

Prof. Henry C. Lord, Director of the Emerson McMillin Observatory, Ohio State University, Columbus, Ohio, U.S.A.

The following Candidate was proposed for election as a Fellow of the Society:—

William James Stewart Lockyer, M.A., Ph.D., 16 Penywern Road, South Kensington, S.W. (Proposed by J. W. L. Glaisher.)

THE BRITISH ASTRONOMICAL ASSOCIATION.

The seventh ordinary meeting of the present session of the British Astronomical Association was held at University College on Wednesday, May 26th, *Mr. N. E. Green*, President, in the Chair.

The election of fifteen new members was confirmed, and the names of five candidates for membership were read and suspended.

Mr. F. J. Wardale read a paper on "The Rotation of Venus." Referring to previous papers and discussions on this question, he pointed out that owing to the proximity of Venus to the Sun the presumption was that a much larger proportion of the water, if any existed on the planet, would be in a vaporous state than on our Earth, and that consequently the probability of the planet's atmosphere being heavily clouded was even greater than had been generally assumed. He next alluded to the extreme rarity of really favourable opportunities for observing Venus. The rising or setting of the planet never differed from that of the Sun by more than four hours, and the result was that it could only be observed under one of three unfavourable conditions:—(1) when the Sun was shining in his strength and setting up air-currents; (2) when the Sun was rising or setting and rapid changes of temperature were taking place with similar results; or (3) when the planet itself was at a low altitude. Mr. Wardale then suggested an experiment for producing artificially the effect of air-currents on a lantern-slide view of the planet. He believed the results would be an appearance of shadings or markings.

Prof. Naegamvala was surprised that no mention had been made of the possibility of settling the question of the rotation of Venus by the spectroscopic method. Two eminent astronomers were now engaged upon the question and he believed it would very soon be settled.

Mr. Maunder remarked that such observations were very much more difficult in the case of Venus than in that of Jupiter. The actual speed of a point on the equator of Venus, supposing a rotation period of 24 hours, would be only something like $\frac{1}{30}$ of the speed they had in the case of Jupiter. The experiment could hardly be undertaken with any hope of success, except with the very largest instruments in existence, and probably not even with those. And this was equally true of the suggested photographic method. The scale of Venus in the primary focus of even the largest telescope would be very small indeed.

Mr. Petrie read a *résumé* of an address delivered by Mr. John

Tebbutt, the President of the New South Wales Branch of the Association, at their second Annual Meeting. The abstract of a paper by *Father Sidgreaves* on "The Spectrum of β Lyræ" was also read. Over 100 photographs had been taken between May and November 1895, but only 88 plates have been employed in the discussion of the changes. These had been arranged according to their time intervals from a preceding principal minimum of the star's light-period, irrespective of their absolute dates. It was found that the bright lines faded considerably during the second half of the light-period, and recovered, abruptly, shortly after the chief minimum, or beginning of the period. But there was one notable exception in the green-blue hydrogen line, $H\beta$. This line appeared to remain comparatively constant in brightness.

Mr. Maunder then gave a short history of recent solar eclipse observation with special reference to the corona, illustrated by a large number of lantern-slides of drawings and photographs.

The President asked Prof. Naegamvala if he had any statement to make with regard to the solar eclipse of January next.

Prof. Naegamvala said there was not much to tell the Association. They knew that the weather probabilities were much more favourable than at Vadsö last year, and the facilities for reaching the line of totality would also be very much more favourable. Prof. Michie Smith proposed to make purely photographic observations. He understood that the Jesuit College of Calcutta intended to send an expedition into the north. His own attempts would be mainly spectroscopic, and he would most likely choose a station near Jeur on the Great Indian Peninsula Railway. Prof. Naegamvala then showed, with the aid of the lantern, several photographs of interesting nebulæ and star-fields taken with a 16½-in. reflector, and also with a Ross portrait-lens of 2 inches aperture and a Dallmeyer portrait-lens 1·75 in. in aperture.

Mr. Davis read a paper on "A Mode of expressing the State of the Atmosphere in connection with Astronomical Observations." He thought it was extremely desirable to distinguish between transparency and disturbance, and submitted the following scale as a useful one for general purposes:—T, perfectly transparent; C, ordinarily clear; D, dense; V, veiled; F, foggy, smoky; S, perfectly steady; M, minimum motion; Z, zephyr; U, unsteady; W, windy; P, passing clouds.

The President thought some agreed system to intimate what sort of opportunity they had was desirable. There might, however, be differences of opinion with regard to the merit of letters or figures.

Mr. Griffiths thought that for most instruments Mr. Davis's scale introduced unnecessary refinement.

Mr. Lunt thought Mr. Davis had made a great step in advance in suggesting a system which divided definition under two headings. The letters arranged by Mr. Davis were a great aid to memory and were preferable to any system which described the state of the atmosphere by figures. Referring to the photographs just shown by Prof. Naegamvala, the speaker said that for delicacy and beauty of detail of the brighter parts of the nebulæ he had never seen anything to excel them.

Mr. Longbottom thought that the simpler the system they adopted the better. Observers differed greatly in their estimation of observing conditions. Mr. Davis's ingenious scheme might prove too elaborate. If the results were good it was self-evident that the conditions were good.

Mr. Maunder said that Mr. Davis had overlooked one point. It was not the amount of wind that was important but the tremulousness of the image. They might have a dancing and flaring image on a night with little or no wind. Mr. Maunder then referred to Prof. Naegamvala's photographs and said he had been greatly struck with the exceeding beauty of the originals, which it had been impossible adequately to reproduce.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE last Meeting of this Society for the present session was held at the rooms of the Royal Astronomical Society, Burlington House, on Wednesday afternoon, June 16th, *Mr. E. Mawley, F.R.H.S.*, President, in the Chair.

A paper by *Mr. R. C. Mossman, F.R.S.E.*, on "The Non-Instrumental Meteorology of London, 1713–1896," was read by the *Secretary*. The author has gone through the principal meteorological registers and weather records kept in the metropolis, and in this paper discusses for a period of 167 years the notices of thunderstorms, lightning without thunder, fog, snow, hail, and gales.

The average number of thunderstorms is 9·7 per annum, the maximum occurring in July, and the minimum in February. The average number of fogs is 24·4, and of "dense" fogs 5·8 per annum. The decadal means show that there has been a steady and uninterrupted increase of fog since 1841. The average number of days with snow is 13·6 per annum. The snowiest winter was that of 1887–8 with 43 days, while in the winter of 1862–63 there is not a single instance of a snowfall. The mean date of first snowfall is November 9th, and of last snowfall March 30th. Hail is essentially a spring phenomenon, reaching a maximum in March and April; the minimum is in July and August. The average number of days with hail is 5·9 per annum.

Mr. C. Harding gave an account of the hailstorm which occurred in the south-west of London on April 27th, 1897. This accompanied a thunderstorm in which the lightning was very vivid. The hail lasted only about 20 minutes, from 6.30 to 6.50 P.M., and in that short space of time the melted hail and rain amounted to about an inch of water. The districts affected by the hail were Tooting, Balham, Streatham, Tulse Hill, and Brixton. The ground was quite white with the hailstones, which in some places remained unmelted the whole of the next day. Much damage was done to fruit and shrubs.

The Great Meteoric Shower of November.

[Continued from p. 244.]

Visible aspect of the Leonids.

THESE meteors display different features according to the varying conditions under which they are presented and according to their size. There is no doubt that the individual particles show a great dissimilarity as regards real dimensions. During an abundant shower of Leonids the smallest meteors visible to the eye are found to be interspersed with occasional fireballs of the largest class. The latter will burst out with lightning-like flashes, illuminate the sky and landscape, and leave dense streaks enduring in different cases from 1 minute to several hours.

Apart from actual differences in size, the meteors exhibit peculiarities of appearance depending upon the elevation of the radiant and upon their apparent position relatively to that focus. When the radiant has just risen the meteors appear to traverse extraordinarily long flights, their directions being nearly parallel to the Earth's surface and presented to the observer under the greatest apparent angles. On the other hand, when the radiant is near the meridian just before sunrise the tracks are much foreshortened by the effects of perspective. With the radiant on the horizon the tracks will be generally 40° or 45° long *, while they will not average more than 10° or 12° when the sickle of Leo is culminating. The radiant of the Leonids passes the meridian at 6^h 30^m A.M. on November 15, at an altitude of 61½° in the S. part of England.

Meteors which appear near the radiant are very short, their lengths decreasing the nearer they are to that centre. Such objects are obviously approaching nearly in the observer's line of sight, and their tracks are greatly foreshortened. A certain number of meteors appear to be perfectly motionless at the absolutely central point of divergence. Under such circumstances they are like fixed stars o somewhat abnormal type, for they have a soft brushy appearance, due to the halo of phosphorescence which surrounds the nucleus and often lingers an appreciable time in the sky after the latter has disappeared. These stationary meteors are sometimes presented simply in the form of small luminous clouds, of round or oval shape and of very transient duration. The Rev. W. R. Dawes, describing his notes of the shower of 1866, Nov. 13, says:—"Several near the radiant-point in Leo appeared like little puffs of steam and assumed the form of a shuttle, which forcibly reminded me of the great nebula in Andromeda as seen with small optical power. These usually remained nearly stationary, though some seemed to twirl round through 20° or 30° while disappearing." Objects of this class are useful as an indication of the place of the radiant; but they cannot always be relied on to show it with great accuracy, for so-called stationary meteors frequently have very short paths of ¼° or ½°, which may be inappreciable to the eye. It is found, as a natural consequence, that, on comparing a number of stationary meteors belonging to a prominent shower, the positions disagree amongst themselves to the extent of several degrees. A few well-observed, long-pathed meteors will allow a radiant to be fixed as exactly as from several supposed stationary meteors; but the latter are perhaps more reliable because they *must* belong to the radiant at or near which they appear, whereas the former being a long distance away *may* emanate from other showers in the same region.

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* On 1879, Nov. 12, 10^h 17^m, when the radiant was just rising, I saw a Leonid with a path 98° long.

The streaks or afterglows visible along the paths of the great majority of the Leonids form a characteristic feature. In every display of this phenomenon observers have been struck with the straight, lance-like films of nebulosity which furrow the sky in all quarters. In some cases these streaks will brighten up perceptibly after the nuclei have disappeared, and they are occasionally seen in places where no meteors of ordinary type have been observed; the inference being, in every such instance, that the afterglow must have been brighter than the head which preceded it. It is not our purpose to enquire into the nature of the appearances alluded to; they may be partly due to luminous cylinders of air produced by violent concussions from the stony fragments piercing it with a velocity of 44 miles per second, or they may be almost entirely attributable to the white-hot material evolved from these projectiles during their rapid dissipation. The fact that the streaks are often broken and visible in sections along the track, while they are always most intense at those particular points corresponding with the greatest outbursts of light in the nuclei, strongly suggests that they are formed by exuded gas from the latter. Moreover the formations referred to are sometimes visible for a comparatively long period and drift many degrees from the places of their first projection in the forms of distinct cosmic clouds, so that we may conclude they are something different to a mere luminous effect temporarily impressed upon the air.

The duration of the streaks varies in different cases, but it is dependent in a great measure upon the size of the meteor producing it. As a rule, the brighter the meteor the brighter will the streak be and the longer its duration. Many of the small Leonids leave streaks which are only of momentary duration, but larger ones equal to Venus, or still larger ones comparable to the Moon in one of her phases, will project streaks for a long period. During the great display of 1866 many of the streaks from fine meteors were watched during 5, 7, or 10 minutes; others were retained in view for 12, 15, and 20 minutes. The writer at Bristol observed a Leonid streak which remained visible during 45 minutes, and several instances were recorded of durations exceeding this. When the streak is first formed it is narrow and perfectly straight, but it soon becomes serpentine and assumes an irregular figure as it drifts * slowly along under the influence of a wind current in the upper region of the atmosphere. Sometimes the form is like the letter S or W, but it changes rapidly during the process of drift and decadence under which its existence is tolerated. The height of these cosmic clouds ranges from about 48 to 60 miles. Their diameter varies from a few hundred feet at first to about 2 miles when they have expanded and curled up into an irregular mass prior to their disappearance.

With reference to the colours displayed by the Leonids and their streaks, it is certain that one description will not apply to all cases. Many of the observers of the last bright return of these meteors have recorded their impressions as to this feature and it may be as well to quote from their reports:—

"The streaks were of a most delicate greenish hue. This greenish tint was very constant. The meteors themselves, on the contrary, had often a ruddy glow, and in cases where the path was much foreshortened to the eye and both streak and meteor could be seen in opposition the contrast between the colours of the two was very remarkable."—G. F. Burder, Bristol.

"The great number of large meteors on the S.E. horizon up to 15° altitude were mostly orange-red, whilst those between Leo and the north were bluish white."—E. J. Lowe, Nottingham.

"Some were of gold or copper tint, some quite ruddy, but the very great majority were brilliant white or blue, resembling the electric light."—T. Crumplen, London.

"The shower consisted of orange-coloured meteors leaving streaks of green."—Sir T. Maclear, Cape of Good Hope.

"The nuclei were mostly bluish-white, except when near the horizon they appeared of an orange or ruddy tint. The trains were generally of a greenish-white hue."—F. Howlett, Canterbury.

"A frequent colour of the nuclei was a ruddy yellow and of the streaks pale-green."—A. S. Herschel, Glasgow.

"Their colour was yellow-orange, and sometimes red, while the

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* Prof. C. Piazzi Smyth observed a fine meteor on 1866, Nov. 13, at 14^h 40^m, which left a luminous track like a silver snake in the sky. This track soon widened and became fainter, drifting also under the action of the N.W. wind blowing at the time. After 15 min. the train matter was still visible, changed to something like the outline of a gigantic pear, and drifted some 30° from its first position. At Athens Schmidt saw a bright Leonid on the same night at 16^h 18^m, which left a visible trail for 51 minutes.

luminous streaks were of an emerald-green or bluish hue, though in some cases red."—V. FASEL, Clapham.

"The colours of the nuclei were mostly white, blue, metallic green, and sometimes a coppery red."—J. COMPTON, Norwich.

"There was a blue or green appearance of several of the streaks, with heads of a ruddy colour. Some few of the heads were also thought to be blue."—JAMES CHALLIS, Cambridge.

"Most of the meteors exhibited a decidedly red head with a bluish-green streak. I noted that their altitude had a great influence on their colour, those on the horizon being much more tinted than those in the zenith, where some of the brightest looked nearly white with blue streaks."—C. GROVER, Chesham.

"The prevailing colour was white, but some of them, especially the larger meteors, had an orange hue, while others again had a bluish tinge. The colour of the streaks was invariably a bright emerald-green."—R. GRANT, Glasgow.

"The metoors were red or white and rarely blue."—R. MAIN, Oxford.

"The nuclei generally red or deep-orange, while the streaks were greenish or bluish."—J. BIRMINGHAM, Tuam.

"The colour may be said to be pale-green, the proportion between this and the red being about 4 to 1."—W. H. WOOD, Birmingham.

The Leonids seen in 1879 by Mr. Corder, at Bridgwater, he described as yellow with green streaks. Of the shower in 1896 he says: "the brightest meteors of all were pale orange or yellow, the rest white, and all left green streaks."

One peculiarity noticed by several observers in 1866 was that the nucleus before vanishing appeared to shoot ahead of the phosphorescent streak, and to pursue its course a few degrees without any attachment. Mr. C. Grover records that in nearly every instance the head ceased to emit a train before it vanished, consequently the head was clearly parted from the streak before disappearance. M. Goulier, in describing his results in the 'Comptes Rendus' for Dec. 1866, says: "the nucleus shot ahead of the streak for some space without emitting the phosphorescent light of the streak." The same feature was independently recognized by Sir J. F. W. Herschel, Mr. R. P. Greg, and others. The explanation probably is that the nucleus which thus loses its streak-producing capacity is merely an exhausted ember or spark in the final stages of its flight. The writer has frequently noticed that under such circumstances the nucleus is fainter and generally moves perceptibly slower than during the earlier portion of its path; in fact, as already stated, the streak-giving power of a Leonid, Perseid, or Orionid meteor is generally in proportion to its brilliancy. This is evident from the fact that upon the course of a long-pathed meteor the streak will exhibit marked condensations, corresponding exactly in place with the positions where the greatest outbursts of light occurred. In cases where a meteor attains its greatest lustre at the extreme end of its course, there also the streak is found to be most intense and durable. But in numerous instances the maximum light seems attained a few degrees before the point of final disappearance, and the nearly spent nucleus is enabled to retain sufficient light and substance to be visible in its path a few degrees beyond the spot where the streak terminates.

The brightness of the meteors forming the Leonid swarm offers another interesting feature for consideration. It has already been stated that the most brilliant type of fireballs are intermingled with the smallest visible shooting-stars during a rich shower. In the display of 1866, Mr. Backhouse obtained the following percentage of magnitudes:—

Number exceeding	1st	mag. stars	7
" equal to	1st	"	14
" "	2nd	"	17
" "	3rd	"	20
" "	4th	"	28
" "	5th	"	14

Mr. J. R. Hind stated that some few of the meteors were of considerable brilliancy, but upon the whole they were not remarkable on the score of brightness. Mr. V. Fasel, at Clapham, said that the greater number of the meteors were very bright and in some instances as large as the planet Jupiter. Mr. S. H. Miller, at Wisbech, Camb., remarked: "with very few exceptions those I registered were equal to stars of 1st or 2nd mag., but some were as bright as Sirius." Mr. G. J. Symons, of London, said that the largest was not twice the apparent brilliancy of Sirius; but this must be a mistake, for a large number of reliable observers describe individual meteors many times brighter than Venus, and in a few cases equal to $\frac{1}{8}$, $\frac{1}{6}$, $\frac{1}{4}$, or $\frac{1}{2}$ the diameter of the Moon. In some cases the firmament and terrestrial objects were brilliantly illuminated by the bursting of large meteors.

Mr. Baxendell, of Manchester, wrote that out of every 100 meteors 10 were above the 1st mag.; the brightest of these were two or three times brighter than Sirius, 15 were between 1st and 2nd mag., 25 between 2nd and 3rd, 30 between 3rd and 4th, 15 between 4th and 5th, and 5 below 5th mag. Mr. W. C. Nash, at Greenwich, said that a far greater proportion of very large meteors of all colours were seen than he remembered to have ever before witnessed. Mr. W. H. Wood, of Birmingham, estimated that the average size was nearly that of Mars, then shining, which many of the meteors resembled. A small proportion only were equal to Jupiter, and he saw only one that exceeded Venus at her greatest brilliancy. Different observers appear to have received very discordant impressions as to the brightness of the meteors. In the case of an individual meteor this can be readily understood, because the various observers are situated at different distances from the object, and the brightness, we know, varies inversely as the

Date.	G.M.T. h	G.M.T. m	Mag.	Position.	Observer and Place.
1865 Nov. 12	12	24	= ♀	At *p* Sceptri from β Cancri.	A. S. Herschel, Hawkhurst.
"	12	44½	> ♀	Path 3° near α Leonis.	" "
"	13	36	= ♀	δ to *E* Leonis.	T. Crumplen, London.
"	17	7	⅔ of ☽	153° +22° to 175° +20°.	S. B. Kincaid, Streatham.
"	17	12	= ☽	156° +26° to 167° +20°.	" "
"	17	16	= ♀	124° +27° to 118° +24°.	" "
"	17	42	3 × ♀	δ Draconis to 95 Herculis.	T. Crumplen, London.
1866 Nov. 13	10	50	3 × ♀	Path 35° below γ Ursæ Majoris.	Observed at Great Yarmouth.
"	11	8	2 × ♀	α–β Geminorum to μ Eridani.	T. Crumplen, London.
"	11	23	¼ of ☽	δ Orionis to ε Eridani.	" "
"	11	30	2 × ♀	Castor to Aldebaran.	" "
"	11	30	¼ of ☽	Castor to Pleiades.	" "
"	11	48	¼ of ☽	α Auriga to 3° above η Tauri.	" "
"	12	6	> ♀	Procyon to 15° above Sirius.	W. R. Dawes, Haddenham.
"	12	40⅔	2½ × ♀	α–γ Ursæ Majoris to ν Ursæ Minoris.	A. S. Herschel, Glasgow.
"	13	8	⅓ of ☽	ι Ursæ Majoris to Cassiopeia.	H. S. Heinecken, Sidmouth.
"	13	27	⅓ of ☽	E. to W., few degrees S. of zenith.	" "
"	14	10	> ♀	Sirius to β Leporis.	S. H. Miller, Wisbech.
"	14	16	> ♀	148° +25° to 135° +20°.	W. H. Wood, Birmingham.
"	14	41	¼ of ☽	Mars and Pollux to θ Tauri.	D. Gill, Aberdeen.
"	15	26	= ♀	β Ursæ Majoris to γ–ν Ursæ Minoris.	S. H. Miller, Wisbech.
1868 Nov. 13	14	51	Fireball.	λ–μ Ursæ Majoris to α Ursæ Majoris.	M. Aquilar, Madrid.
"	16	40	2 × ♃	In S.S.E. from α Leonis.	W. C. Nash, Greenwich.
"	16	12	Fireball.	In Pisces low in W.S.W.	T. P. Gillman, Newhaven.
"	21	59	"	Passed 1° S. of β Geminorum.	H. A. Newton, Newhaven.
14	12	38	"	Between β and ψ Ursæ Majoris.	M. Aquilar, Madrid.

square of the distance. Thus a splendid Leonid fireball burst out over the neighbourhood of Dundee on 1866 November 13 14^h 40^m, and the various estimates of brilliancy were as follows:—Nottingham: 8× ♀. Oundle, Notts: "Exceedingly large fireball, lit up the whole heavens as with the light of day." Edinburgh: Large meteor, cast the observer's shadow on the ground. Newcastle on Tyne: 3× ♀. Glasgow: Very large meteor, must have been nearly as bright as the full Moon, for it lighted the interior of the observatory. Aberdeen: one-fifth diameter of full Moon. As regards the general brilliancy of the meteors forming the shower Prof. A. S. Herschel said three appeared brighter than ♀, and one very much brighter. Prof. Grant noted that "Multitudes of the meteors were equal in brightness to stars of the 1st mag., many of them were equal to Jupiter when in opposition, while some of them rivalled Venus when at her greatest brilliancy.

The table on the opposite page gives a few selections of brilliant meteors seen during the last epoch of the Leonids.

In 1868 many large Leonids were also observed. At Greenwich, on the night following Nov. 13, a fine shower was witnessed and 18 of the meteors were as bright as or brighter than Jupiter. As further evidence of the great lustre of the Leonids, it may be stated that during notable returns a few of the meteors have been observed after sunrise. Thus on 1867 Nov. 14 between 8 and 8.30 A.M. several meteors are alleged to have been seen at Bremen crossing the Sun's disc, then low on the horizon.

The meteors are not accompanied by any detonation or audible sound. Several observers have, it is true, asserted that they have heard a hissing or crackling noise at the times when some of the largest meteors have fallen, but this must have resulted from an erroneous impression. The phenomenon of a great meteor shower is an absolutely silent one, though the evidence of violent commotion and ceaseless activity is written in vivid characters upon the sky. It is to be presumed, however, that, in the very exceptional case where a fine Leonid approaches comparatively near to the Earth's surface before its final disruption, there is no reason why a perceptible detonation should not reach the ears of spectators. Such an effect may be regarded as quite possible, for detonating fireballs from other systems are not very rare, and there is no reason to suppose that the Leonids belong to a special class of meteorites either in respect to their composition or distances. The fact remains, however, that in the presence of the great activity and brilliancy of past Leonid displays the silence of night has been rarely, if ever, broken *.

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* During the feeble shower of Leonids on 1895, Nov. 13, Mr. Blakeley, at Dewsbury, saw a very fine meteor (about half equal to the Moon in apparent brightness). The time was 12^h 32^m, and the apparent path of the meteor from $215^\circ +55^\circ$ to $230^\circ +55^\circ$. At 12^h 43^m he heard a dull, faint reverberation from all round the sky, and thought that, in the absence of any other explanation, the sound must have been caused by the explosion of the meteor.

W. F. DENNING.

[To be continued.]

Confirmed or New Variable Stars.

From my observations I can confirm or report the following orange or red stars as certainly variable, though they do not appear in Chandler's 'Third Catalogue of Variable Stars.' Some of these stars have been announced as suspected or certainly variable by other observers.

1. Es. Bm.	2. Bm.	3.	4. R.A. 1890.	5. Dec. 1890.	6. Mag.	7. Average colour.	8. Spectrum type.
			h m s	° ′			
153	121	Taurus.	5 39 6	+20 39	7·7	$\mathrm{OrR}^{4\cdot 2}$	IV.
172	135	Gemini.	6 4 3	+26 2	7·4	$\mathrm{OrR}^{4\cdot 0}$	IV.
173	136	,,	6 5 15	+21 54	7·3	$\mathrm{ROr}^{3\cdot 2}$	III.
350	277	Virgo.	12 19 37	+ 1 23	8·1	$\mathrm{ROr}^{4\cdot 6}$	IV.
492	410	Ophiuchus.	17 23 14	−19 23	7·8	$\mathrm{R}^{4\cdot 3}$	IV.
735	—	Lacerta.	22 43 36	+54 38	9·5	$\mathrm{ROr}^{4\cdot 0}$*	III.

In the table col. 1 gives the number in Espin's Revision of Birmingham's Catalogue of Red Stars; col. 2 Birmingham's number; col. 3 the constellation; cols. 4 and 5 position for 1890; col. 6 B.D. magnitude; col. 7 the average colour from my observations. In this column the names of the colours are nearly the same as those given by Franks in the 'Monthly Notices of the Royal Astronomical Society' (vol. xlvii. p. 271), except that two tints are taken between the principal colours; thus between Or and R I take ROr (reddish orange) and OrR (orange-red). Col. 8 gives the type of spectrum as given by Krüger in his 'Catalog der farbigen Sterne.'

All of these stars are of Pickering's III. Class of variable stars, unless it be the star in Lacerta, which may be of Class IV.

Bm. 121 is of Espin's 19 Piscium type, which appears to be a subclass of Pickering's Class III. Its range is about two magnitudes. It has long, I believe, been recognized as a variable by others besides myself.

Some years ago I announced Bm. 135 and 136 Geminorum as probably variable. I am satisfied as to their variability. An examination of my observations shows that though Bm. 136 is as a rule steady, or nearly so, it is sometimes decidedly variable, its range being from 6·7 to 7·9 magnitude. Its spectrum being stated to be of Type III., it does not come under Espin's 19 Piscium type of variable; though whether the spectrum should be held to settle the classification as to variability is a moot point. If it should in some cases, should it not in all, and so cause a further subdivision of the classification?

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* Es. Bm. 735 is midway between $\mathrm{Or}^{4\cdot 0}$ and $\mathrm{ROr}^{4\cdot 0}$.

The fluctuations of Bm. 135 have, on the other hand, an approach to regularity. A period of 443 days satisfies the observations better than any other, and fits most of them, but is entirely inconsistent with some. The period is therefore clearly irregular, yet apparently too regular to class this star under the 19 Piscium type. The range is from 7·4 to 8·2 magnitude.

So far as I have yet observed, Bm. 277 and 410 have a small range, and are likely to prove of the 19 Piscium type. The red colour of the latter, though not so deep as of some other stars, has usually, though not always, appeared to be without the slightest admixture of orange.

Es. Bm. 735 probably has a wider range. It is usually too faint to show much colour with my instrument (a 4¼-inch refractor).

In your issue of February 1895, I announced Bm. 18 Piscium as probably a variable, but my observations have shown it as practically steady since then.

Sunderland, 1897, June 23. T. W. BACKHOUSE.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

British Observatories Sixty Years ago.

GENTLEMEN,—

So much has been said and written in this country lately with the object of comparing the state of things now and at the Queen's accession in 1837, that perhaps a few words may be of interest with regard to the observatories which existed in the British Islands and Colonies at the date in question. Remarks will be unnecessary on the progress made since.

In that year Airy had but recently been appointed Astronomer Royal, and had not yet effected any changes of importance in the instrumental equipment of the Greenwich Observatory. Challis had succeeded him at Cambridge, and the Northumberland equatorial there was the largest telescope in use in England. The Radcliffe Observatory at Oxford was under the direction of Rigaud, but no observations of his have been published, and systematic practical work did not commence until the appointment of Johnson in 1839.

The Edinburgh Observatory on Calton Hill had become Royal in 1834, and Henderson was as active there as his health allowed, both in actual observing and in the reduction of the observations which had been made by him at the Cape of Good Hope. No other observatory then existed in Scotland.

Sir W. R. Hamilton had succeeded Brinkley at Dublin in 1827, but it does not appear that any practical work was carried on at the Dunsink Observatory in his time. Dr. Robinson, however, was energetically pursuing his stellar work at Armagh.

The English private observatories in activity in 1837 were those of Lord Wrottesley at Blackheath, Lassell at Starfield, and Dawes at Ormskirk (where he was observing double stars). Dr. Lee had re-erected at Hartwell in 1836 the telescope which Admiral Smyth had used at Bedford, but activity did not commence at the former place until the appointment of Epps as assistant about the end of 1837. Bishop's observatory in Regent's Park (which afterwards became so famous) had been founded the year before, but observing work did not practically commence there until Dawes removed thither in 1839. On the other hand, Hussey's observations at Hayes Rectory had recently terminated at the epoch of which we are speaking, the last recorded (excepting meteorological) being of Halley's comet. But in Ireland, Col. Cooper's observatory at Markree Castle, co. Sligo, had been in activity for about three years, and possessed the largest refractor in the British Islands.

The Cape Observatory was under the direction of Maclear, who had been appointed to it in 1834. He landed ten days before J. Herschel, who set up his telescope at Feldhausen, and made during the next four years those memorable observations of the nebulæ &c. in the southern hemisphere which so well supplemented his father's work in the northern. Taylor was making meridian observations at Madras, but it is to be feared that astronomy has not benefited much by them. The Brisbane Observatory at Paramatta was still in existence, but its activity had ceased about two years before the time of which we are speaking. Regular astronomical work did not commence in Australia until sixteen years afterwards, when the observatory was founded at Williamstown, which ten years later was transferred to Melbourne.

Yours faithfully,

Blackheath, 1897, June 3. W. T. LYNN.

P.S.—May I take the opportunity of pointing out a slip or misprint in my letter of May 6? At p. 247, line 10, for "and of Mr. Hind" read "out of Mr. Hind." As it stands it gives the impression that Hind had been lecturing at Whatley, whereas the meaning is that Dean Church had derived his information from what Hind had written.—W. T. L.

White Spots on Jupiter.

GENTLEMEN,—

The white spots described by Herr Brenner on p. 248 of the current number of the 'Observatory' are very interesting, but instead of their having remained in the same position with respect to the red spot (or the "shoulder"), they evidently in reality partook of the westerly drift relative to the latter, which is peculiarly characteristic of the surface-markings in the locality of the South Temperate Band. Take, for instance, the observations of the first spot of 1897 given by Herr Brenner, the positions of which are based on micrometer measures *. Between March 21 and May 3 the longitude of this spot decreased by 26°, or at the rate of ·605° per day. This corresponds to a period of rotation of $9^h\ 55^m\ 15^s{\cdot}8$, which is in excellent agreement with previous determinations of this element †. A similar motion is shown by the second spot observed by Herr Brenner this year. Taking again the positions based on micrometer measures ‡, the longitude of the spot decreased by 15° between April 2 and May 3, or at the rate of ·484° per day. This corresponds to a rotation period of $9^h\ 55^m\ 20^s{\cdot}8$, which is likewise in excellent agreement with the foregoing.

It seems certain, therefore, that the presence of the white spots above the left "shoulder" of the S. equatorial belt referred to by Herr Brenner was purely accidental. As a matter of fact, such white spots are exceedingly numerous in this latitude, so that as they drift slowly past the red spot it is a matter of frequent occurrence for one to occupy a position approximately above the "shoulder." But always, whenever any individual spot is adequately observed, it will be found to have this westerly drift, which has, in fact, been more constant and uniform than the drift of the surface material in any other latitude, the different determinations being in remarkably close agreement ever since the first one by Schroeter in 1787.

The westerly drift of these white spots, I may say, is well shown by some of Herr Brenner's own drawings and charts. See, for instance, charts vii., x., xi., xiii., and xv. of his magnificent recently-published work, 'Jupiter-Beobachtungen 1895–6 an der Manora-Sternwarte,' where the westerly drift of a curious double white spot, above the preceding end of the red spot, is admirably shown.

Yours faithfully,

Hove, 1897, June 17. A. STANLEY WILLIAMS.

The Orbits of Meteorites.

GENTLEMEN,—

In the current number of 'The Monthly Notices' of the Royal Astronomical Society, Mr. Denning concludes from observations of fireballs that they are usually, if not always, moving in orbits of short period with direct motion, like the Jovian class of comets. Some years ago the late Prof. H. A. Newton deduced a similar conclusion as regards meteorites, from an analysis of the narratives relating to their falls. Indeed the connexion between fireballs and meteorites cannot be doubted, for sometimes a fireball has exploded and fragments have been picked up.

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* The two observations of Feb. 24 and March 8 evidently relate to a different spot.

† In the 'Monthly Notices,' vol. lvi. p. 149, I gave $9^h\ 55^m\ 18^s{\cdot}1$ as the average rate of rotation of the surface material in this latitude.

‡ Positions derived from drawings are seldom sufficiently accurate to be of any value for the purpose of such determinations.

In this state of things, it seems by no means improbable that the meteorites with which Sir J. N. Lockyer and others experimented had always formed a part of the solar system, and that none of them were visitors from external space. If so, the Meteoritic Theory rests upon rather an unstable basis.

It is of course possible that meteorites and fireballs may be visitors from space, which were captured by one of the planets—in most cases Jupiter. But I do not think this capture-theory affords a satisfactory explanation of the facts. Why, for instance, should the motion of the captured body be direct? Or, at least, why should retrograde periodic comets and meteors always have periods long enough to assimilate their case to that of the satellites of Uranus?

Yours truly,

Dublin, 1897, June 23. W. H. S. MONCK.

*A new * Division in the Saturnian Ring-System.*

GENTLEMEN,—

I have the honour to inform you that Mrs. Manora has discovered a new division in the Saturnian ring-system. It separates the ring B from ring C, and is as black and nearly as broad as the Cassini division. When I observed and measured it first (June 2) I saw it at once quite distinctly, as I did also on June 6 and 8, when I measured it again. On June 5 the new Manora division was invisible on the right ansa and indistinct on the left one, so that I suppose it can be seen only under good atmospheric conditions, on account of the darkness of the crape ring, which is much darker now than it was in the preceding apparitions. There is not the slightest doubt but that the Manora division is really a recent formation; for it is quite impossible that I should not have seen it during the last apparition on many occasions, and I remember quite well that under the *best* conditions the rings B and C were *not* separated by a dark line. Besides, Mrs. Manora had examined particularly the rings since the discovery of the Antoniadi division, in view of finding perhaps another one—a hope realized indeed! The measures gave 0″·56 for the Manora division and 0″·62 for the Cassini division, while I estimated the breadth of the Encke and Antoniadi divisions (both visible on the 2nd, 6th, and 8th inst.) about 0″·10–0″·15. (I need not mention that the Manora division is *not* identical with the Struve division.)

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* [We scarcely think that Herr Brenner is justified in calling this appearance a *new* division, for so many markings of this kind have been seen on Saturn's rings at different times, that it leads one to think that these may often be subjective phenomena, or, as was suggested by M. Flammarion at the time when M. Antoniadi observed a similar appearance (1896, April), it may be that the action of the satellites on the molecules composing the rings causes periodic displacements, which are seen as divisions. On 1883, October 29, Prof. Davidson saw the division of the crape ring visible all round ('Observatory, vol. vii. p. 85), and there was an undoubted difference in brightness of the dusky ring at the two ansæ. This observation seems to be similar to that of Mrs. Manora.—EDS.]

Yours faithfully,

LEO BRENNER.

Manora Observatory, Lussinpiccolo,
1897, June 17.

OBSERVATORIES.

ROYAL OBSERVATORY, GREENWICH.—The Annual Visitation took place on June 5, and with the exceptions of Lord Lister and Capt. Abney, who has been elected a Visitor in place of the late Mr. Stone, the complete Board of Visitors attended to receive the Astronomer Royal's Report, from the various sections of which the following information is extracted. The period covered is from 1896 May 11 to 1897 May 10 inclusive.

Transit Circle.—The number of observations of transits made was 11,963, of zenith distance 10,421. The latter number includes 509 observations of stars by reflexion and 643 of nadir-point. The annual catalogue for 1896 contains 3454 stars. The discordance between the reading for nadir-point as determined by reflexion star observations, and by observations of the reflected horizontal wire, which has been comparatively large since 1892, was larger in 1896, and this led to an examination of the screws of the microscope micrometers and of the telescope micrometer, and to a re-determination of the division errors used for the nadir observations. The use of these new errors, and the deduced corrections to the value of one revolution of the telescope-micrometer screw at the end of its range, diminished the discordance to −0″·34. A remarkable fact, which is, however, given with some hesitation as to its validity, is that the reading for the nadir-point is different as determined at different times of day.

The correction for R−D agrees closely with the value given in 1895. The correction to the tabular obliquity of the ecliptic is −0″·07.

Altazimuth.—The total number of places of the Moon observed with this instrument was 55, or 4·4 per lunation. The new Altazimuth has given considerable trouble in its construction and is now not finished. At first a flexure in the axis had to be corrected by means of a strong diaphragm in its central part, and quite recently Mr. Simms has detected an unexpected source of error. Owing to the method of grinding the pivots, they have a slight tendency to act as screws on the bearings and so draw together the standards which support the instrument. This difficulty has now been overcome and it is confidently hoped that work with this instrument will be shortly begun.

The Thompson Equatorial.—This instrument, presented by Sir [illegible] Thompson and made by Sir Howard Grubb, has been mounted [illegible] the top of the Physical Observatory under the Lassell Dome.

mounting is of the German form, but permits complete circum-polar motion, and is such that no reversal is necessary at the meridian. At one end of the declination axis is the photographic telescope of 26 inches aperture and 22 feet 6 inches focal length, the 12¾ Merz, formerly the telescope of the Great Equatorial, which is used as a guiding telescope, and the Thompson 9-inch photo-heliograph. At the other end of the axis there is a Cassegrain reflecting telescope of 30 inches aperture, the mirror of which was ground under the direction of Dr. Common, with the 6-inch Hodgson telescope as guider. The position of the polar axis has been adjusted, experimental photographs of stars have been taken to determine the proper separation of the lenses and the position of the focus, and also some satisfactory photographs of the Moon, both in the primary focus and with a secondary magnifier. A photographic spectroscope has been provided, and since the date of the Report has been mounted at the back of the mirror of the Cassegrain. A prism will be used to reflect the rays into the collimator of the spectroscope.

The 28-inch Equatorial has been used almost continuously throughout the year for micrometric measures of double stars and the dimensions of the planetary systems. For a short period this instrument was used for photographing double stars, the crown lens being reversed for this purpose. The record of work with the Astrographic Equatorial is summed up by saying that 135 successful plates have been taken for the Chart, and 110 for the Catalogue, which completes almost half of the Chart plates and rather more than two-thirds of the Catalogue plates of the Greenwich section.

The Sun-spot record has been continued as in previous years. Supplemented by photographs from India and Mauritius, photographs are available for 360 days out of 366.

No spectroscopic work has been done during the year.

Magnetic and Meteorological Branch.—It has been determined that a pavilion to contain the instruments for determination of absolute magnetic elements shall be built in the Park, but at such distance from the Observatory that the results may be unaffected by the masses of iron which necessarily exist in the instruments and structures of the parent building. Much consideration has been given to the selection of a site for this, and a place has been chosen to the east of the Observatory.

A curious idiosyncrasy has exhibited itself in the Campbell-Stokes Sunshine recorder. It was pointed out by Mr. Marriott that comparison of the sunshine record at Greenwich with those made at Kew, and at Bunhill Row in the City, showed that the amount of sunshine registered at Greenwich had been gradually diminishing during the last six years. The suspicion that this was due to a defect in the glass has been proved to be correct by making a record with a new ball side by side with the old one, when the defect of the old ball was quite apparent.

The mean temperature of the year 1896 was 50°·1, being 0°·7 above the average. The rainfall for the year ending 1897 April 30 was 26·83 inches, being 2·29 inches above the average. The number of rainy days in the twelve months was 178.

The rating of British and Indian Government chronometers has been continued as hitherto, as well as the trials of chronometers and deck watches sent in by makers.

ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—Dr. Gill's report for the year 1896 opens with a reference to the McClean telescope and the observatory to contain it. The building is finished, and the dome put on so far as is possible until the heavy metal castings of the telescope pier are put in their places. The machinery of the hydraulic rising floor is in process of erection*. Delay has occurred in the construction of the telescope, but Dr. Gill expects that the whole installation will be completed and in working order by the end of 1897.

The Director's policy during recent years has been to limit the number of observations made with the transit-circle in order to cope with the arrears of reduction and publication. During 1896 it has been possible to actively resume meridian work, but restricting this to night-work only. The Sun has not been observed nor the planets by day. 9000 transits have been observed during the year, and 8677 determinations of zenith-distances made, which speaks highly for the energy of the observers. 86 phenomena of occultation of stars (including Venus) by the Moon have been observed during the year, and this, except for the photographs taken with the astrographic telescope, is the total of the observing work. Fifty-five plates have been taken for the Astrographic Catalogue, and 244 for the Chart, besides 220 for determination of stellar variability and for other purposes. The Catalogue plates are now all taken, but many of these will have to be replaced, on account of paucity of stars shown. Of the 244 Chart plates, 174 have been passed as satisfactory. This completes nearly half the number allotted to the Cape Observatory. 26 Catalogue plates have been measured completely, each by two observers.

Mr. Innes, who is Secretary and Librarian of the Observatory, has also undertaken observations with the 7-inch equatorial with much success. He has examined 218 stars of a list given as missing on the plates of the Cape Photographic Durchmusterung, and has proved 8 of these to be new variables, and has otherwise accounted for the absence of the others. He has also discovered 104 new double stars. Three new double stars have also been detected by the transit-circle observers.

The arrears of publication are being steadily cleared off. The Geodetic Survey of South Africa' and vol. i. of the 'Photographic Durchmusterung,' which have been published during the year, have already been noticed in these pages. Vol. ii. of Dr. Gill's Solar Parallax work is ready for distribution, and vol. i. is far advanced, as is also the second volume of the 'Durchmusterung.'

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* The Report is dated 1897, January 15.

A Cape 10-year Catalogue of 3007 stars for equinox 1890 is promised shortly.

Dr. Gill notes that the Governing Authorities have favourably considered his proposals for an increase of staff and for the provision of a reversible transit-circle, and trusts that these proposals will soon be carried into effect.

UNIVERSITY OBSERVATORY, OXFORD.—The Report of the Savilian Professor to the Visitors, for the twelve months ending 1897, May 31, relates mainly to the Astrographic Catalogue, which is the staple work of this Observatory.

Not many plates have been taken during the year, as it is at present considered more expedient to concentrate the energy on the measurement and reduction. By help of a grant of money from the Royal Society, Prof. Turner has been able to employ four or five computers, who have been engaged in measuring plates during a considerable portion of the year. The number now completely measured is 155 out of 1180. Besides this a considerable amount of reduction has been effected; in fact it is estimated that about one-fifth of the whole work is done, and it is not unlikely that the Catalogue may be completed by May 1901.

Prof. Turner is putting into actual reality the proposals for a photographic transit circle, which he laid before the Astronomical Society in March last. The twelve-inch mirror presented by Dr. Common will be an essential part of the instrument, which, if successful, will be used for determining the places of the fundamental stars for the reduction of the Astrographic Catalogue.

The general impression that the Report gives the reader is that the University Observatory needs a good deal of money spent on it, both for structural improvements and for instruments. Something appears to have been done by way of repairing roofs and domes and internal decoration, but these repairs are evidently only temporary. Last year the Board of Visitors approved a proposal that a house should be attached to the Observatory; but the University authorities have not seen their way to carry this into effect. The writer of this note, who has wasted much time and energy in walking fruitlessly from home to telescope on semi-cloudy nights, can quite understand the Director's disappointment at the rejection of his plan, which was one not entailing much cost to the University.

PUBLICATIONS.

HARVARD COLLEGE ANNALS, VOL. XXVIII., PART I. SPECTRA OF BRIGHT STARS.—The work before us, another of the long series published by Prof. Pickering, contains the results of the examination of 4800 photographs which represent the spectra of 681 of the brightest stars north of Dec. $-30°$. Prof. E. C. Pickering assigned this work, which forms part of the Henry-Draper Memorial, to Miss Antonia C. Maury in 1888, and we congratulate her on the completion of a most important and interesting research.

The telescope, 11 inches aperture and 153 inches focal length, was made primarily for visual work, but it can be adapted for photography by the addition of another lens. The spectra are obtained by objective prisms, of which there is a battery of four, so arranged that any combination of them can be placed before the object-glass. Generally speaking the length of exposure was one hour, and the spectrum between $H\epsilon$ 3970 and $H\beta$ 4861 was 8·0 cm. when the four prisms were used, and 2·0 cm. when only one was used.

To determine many of the wave-lengths of the lines in the typical spectra, the solar spectrum was photographed with the same telescope, using the 15-inch Draper reflector as a collimator. As the classification of the spectra must to some extent be dependent on the appearances, as well as on the positions, of the spectral lines, care was necessary in differentiating between the appearances due to faulty focal adjustment, over- or under-exposure, or of the appearances of lines characteristic of particular stars, and from this cause it was sometimes necessary to examine a considerable number of good photographs before a satisfactory decision as to the proper place of a spectrum in the classification could be arrived at. This classification, which is done in the most exhaustive way, is too elaborate to describe closely within the limits of this short review, but an idea of it may be formed from the table which we have copied later. The stars were arranged in a progressive series of twenty-two groups. The first group contains stars of the most pronounced Orion type; the next four contain stars which may also be considered as Orion stars, and the groups which follow similarly contain in order stars of the five usual spectrum types or stars whose spectra may be considered intermediate to these. But as this system of grouping appeared inadequate to represent all cases, parallel columns were added in which the stars of each group were distributed in "divisions" common to each group according to the criteria described next following. The chief division, *a*, includes 355 stars, of which none of the single lines, that is lines which cannot be resolved into pairs or groups by increasing the dispersion, are relatively wide, except those of hydrogen and calcium, *e. g.* *α* Lyræ, *α* Canis Min., the Sun, *α* Boötis. Division *b* includes those stars in which the lines are relatively wide and hazy, *e. g.* Ursæ Maj., *β* Aurigæ, *α* Aquilæ, *α* Leonis, *δ* Orionis. In division *c* the hydrogen lines are narrow and well defined, the Orion lines are also narrow and well defined, the calcium lines are more intense, the system of metallic lines contains some apparently not coincident with any lines of the solar spectrum. In general the spectra in division *c* have lines narrow and of a strongly-defined character, those in division *b* have very wide lines, while those of *a* are intermediate.

Type.	Group.	Division.								Total.	D.C.
		c.	*ac.*	*a.*	*a,b.*	*ab.*	*b.*	*a, b, ac.*	*p.*		
o.	I.	...	...	...	...	...	7	...	...	7	B.
o.	II.	...	...	4	5	...	5	...	2	16	B.
o.	III.	1	...	5	7	...	5	...	1	19	B.
o.	IV.	...	...	11	22	3	14	...	...	50	B.
o.	V.	3	...	9	8	...	5	...	...	25	AB.
o, I.	VI.	6	...	4	9	3	9	...	...	31	AB.
I.	VII.	1	...	13	13	...	17	...	1	45	A.
I.	VIII.	1	1	23	6	3	17	...	7	58	A.
I.	IX.	...	...	?	17	3	9	5	...	34	AF.
I.	X.	...	...	2	12	2	3	...	...	19	AF.
I.	XI.	...	5	?	11	5	...	7	1	29	F.
I., II.	XII.	2	3	29	...	1	...	...	...	35	FG.
II.	XIII.	4	1	22	...	...	...	...	...	27	G.
II.	XIV.	...	7	40	...	..	...	...	3	50	G.
II.	XV.	...	...	117	...	...	...	...	1	118	K.
II.	XVI.	...	...	23	...	...	...	...	...	23	K.
III.	XVII.	...	...	19	...	...	...	...	...	19	M*a*.
III.	XVIII.	...	...	20	...	...	...	...	...	20	M*b*.
III.	XIX.	...	...	10	...	...	...	...	...	10	M*b*.
III.	XX.	...	...	4	...	...	...	...	2	6	M*d*.
IV.	XXI.	...	...	...	...	...	...	...	...	4	N*a*.
V.	XXII.	...	...	...	...	...	...	...	...	4	O.
C.	......	...	...	...	...	...	...	...	...	18	
L.	......	...	...	...	...	...	...	...	...	14	
	Totals...	18	17	355	110	20	91	12	18	681	

Spectra which might be assigned either to *a* or *b* are found in column *ab*. Peculiar spectra, and such as might belong to any division, will be found in column *p*, while D.C. indicates the classification in the Draper Catalogue (Vol. xxvii. 'Annals of Harvard'). "While it will be generally admitted that the series represents successive stages in stellar evolution, it may still be doubted whether the arrangement beginning with the Orion type, and here adopted, is in fact the natural order. It is strongly indicated, however, by the gradual falling off of the more refrangible rays in the successive groups, by the corresponding increase in the less refrangible rays, and by the occurrence of marked absorption at the close of the series. The comparative simplicity of the Orion spectra and the increasing complexity shown throughout the series lend additional weight to the argument. Finally, the prevalence of the Orion type in great nebulous regions, as in Orion and the Pleiades, indicates very emphatically that stars of this type are in an early stage of development."

Stars designated by the letter *c* seem to combine features of widely different types, the most common being where a spectrum of the second or third type is associated with a hydrogen spectrum that is characteristic of the first type. Some of these, such as γ Andromedæ and 31 Cygni, are known doubles, and the strong resemblance suggests that these anomalous spectra may in general be those of close doubles. In these spectra the K-line appears either unduly narrow or overspread with a peculiar haziness, due probably to the presence of an additional star, having a spectrum belonging to some group earlier in the series. It is also significant that in such spectra the first type characteristics predominate in the ultra-violet, the second or third type in the green and blue. This is exactly what would happen if two spectra of the first and second type were superposed. Perhaps the most beautiful examples of the composite type are ζ Aurigæ and δ Sagittæ, which combine the Orion type with the third and first types respectively. Each group and division is then treated of in detail and full tables given, with copious notes to each star. To make the whole as complete as possible a general or working catalogue is added, with the stars arranged in order of R.A., with references to their numbers in the Harvard Photometry, Baily's Flamsteed, and the Bonn or Cordoba Durchmusterung. Further columns give the group and division and reference to remarks in the present volume.

With this catalogue one naturally expected to have exhausted the contents; but evidently thoroughness has been the leading idea, for we find an additional six pages devoted to "helium" lines, the recent discovery of which precluded reference to these in the main body of the work. If this discovery had been made earlier the plan of the classification might have been altered, and some of the conclusions might have been modified. No less do we congratulate Miss Maury on the observational work on which her results depend, than on the way the results are discussed and presented. The introductory Chapter I. is a brief but intelligible essay on stellar spectroscopy.

FROM a posthumous paper, entitled "an Account of an Investigation by the late Joseph Baxendell, F.R.S., &c., as to Short-period Cyclical Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface," recently read before the Liverpool Astronomical Society, we learn that the author in the course of an investigation of the irregularities in the changes of some of the variable stars—having been induced "to regard it as highly probable that the light of the Sun, and also its magnetic and heating powers, might be subject to variations of a more complicated nature than had been hitherto supposed, and that, besides the changes which are indicated by the greater or less frequency of solar spots, others of a minor character, and occurring in shorter periods, might also take place,"—undertook a laborious discussion of a series of magnetical observations made at the Imperial Observatory of St. Petersburg and of several series of thermometrical observations from various observatories with a view of detecting these minor changes. It appeared to Mr. Baxendell to be established from this investigation that the period of cyclical change of the element involved exhibited a variation, diminishing gradually for a series of years and afterwards gradually increasing, to diminish again after it had completed its cycle of change, the maximum and minimum values of the period being respectively 31 and 23½ days, and it was at once inferred that this variable period was in some way connected with and dependent upon the solar spot period, the minimum value occurring in the year of minimum frequency of solar spots and *vice versa.*

These facts being established, the author propounded the following views which he considered might explain them, viz.:—

(1) "That a ring of nebulous matter exists differing in density or constitution in different parts, or several masses of such matter forming a discontinuous ring, circulating round the sun in a plane nearly coincident with the plane of the ecliptic, and at a near distance from the Sun of about one-sixth of the radius of the Earth's orbit."

(2) "That the attractive force of the Sun on the matter of this ring is alternately increased and diminished by the operation of the force which produces the solar spots, being greatest at the times of minimum solar spot frequency, and least when the spots are most numerous."

(3) "The attractive force being variable, the dimensions of the ring and its period of revolution round the Sun will also vary, their maximum and minimum values occurring respectively at the times of maximum and minimum solar spot frequency."

The theory is, that this ring acts not only by reflecting and absorbing a portion of the light and heat which would otherwise reach the Earth, but also by altering the direction of the lines of magnetic force.

We give this theory not because it is supported by any evidence so strong as such a hypothesis would seem to require, but merely as the expression of an idea similar to one which occurred to Leverrier as a reason for the excess in the motion of the perihelion of Mercury, but which results from a perfectly different line of thought. We see also from the paper before us that the late Dr. Joule had expressed a favourable opinion of Mr. Baxendell's hypothesis.

This account was communicated to the Liverpool Astronomical Society by the son of the author, Mr. Joseph Baxendell, who is now the official meteorologist to the Southport Corporation. He has lately sent us his Report of the Fernley Meteorological Observatory for 1896, with the results of observations, which give evidence of much care and completeness.

NOTES.

Comet Notes.—'Bulletin Astronomique' for May contains a comparison by M. Schulhof of the observations of Denning's Comet (1894 I.) with the places computed from different systems of elements. He regards his fourth system of elements (given in Ast. Nach. 3276) as the most accurate yet published, but as capable of some further adjustment. He does not regard the theory that this comet is a fragment of Brorsen's as a probable one. It would be necessary to suppose a shock sufficiently great to alter the inclination of the orbit by 24°, and to increase the velocity from 4·7 to 6·6 miles per second.

M. Coniel gives a search ephemeris of Swift's Comet (1889 VI.) in the same publication. He recently found that the most probable period of this Comet was 8·9 years, but with a probable error of 0·9 years either way. He makes several different assumptions as to the date of the next perihelion passage and gives corresponding ephemerides. As an abridgment of these would be of little use, those who wish to search for the Comet should refer to the original paper.

Ast. Nach. 3427 contains a useful series of observations of Comet 1897 I. (Perrine), made by Mr. John Tebbutt at Windsor, N.S.W. They range from 1897 Feb. 23 to April 20. The Comet did not become so bright as was anticipated, and on April 20 it was of the last degree of faintness, in spite of a clear sky.

A. C. D. C.

Obituary.—Alvan G. Clark *. Prof. Alvan G. Clark, maker of the most famous lenses of telescopes in the world, died in his home here (Cambridge, Mass.) to-day. Apoplexy was the cause of his death. He had been ill for several months, but had journeyed to Chicago to direct the placing of the great lens he had made for the Yerkes Observatory. The fatigue of the journey prostrated him, but it was expected that he would recover.

Prof. Clark was an American of the ninth generation. He was a direct descendant in the male line of the Thomas Clark who was mate of the Mayflower. His father founded the house of Alvan Clark and Sons, telescope-makers, when he was forty years old. Up to that time the elder man had been a portrait-maker, and his work as an artist is to-day celebrated for likeness and perfect finish.

Alvan G. Clark was born in Fall River, Mass., on July 10, 1832. He was educated in the public schools of Cambridge, whence the family had moved. When twenty years of age he was taken into partnership with his father and brother. He, too, as a boy, showed an aptitude for everything requiring keen vision and exactness. He was also deeply interested in astronomy while still at school, and gained prizes for treatises on casting and grinding specula. He has carried on the business since the death of his father and brother, and under him Clark and Sons has achieved its greatest fame.

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* From the 'New York World,' June 10, 1897.

While testing lenses long ago the senior Clark discovered several double stars, and when completing an 18-inch glass for the Chicago Observatory Alvan G. turned his glass upon Sirius and found a companion to that star. For this discovery he was awarded the Lalande medal by Prof. Chacornac of the French Academy of Sciences. Numerous other astronomical discoveries, including sixteen double stars, followed, and the fame of the Clarks was increased by the sale of telescopes in Europe and America.

The first order received by the firm for a big lens to be used in this country was in 1860 for an 18½-glass for the University of Mississipi. Another came in 1862 for a glass for the Astronomical Society of Chicago, aud it was with this glass that Alvan G. sighted the companion to Sirius.

When the Clarks had proved that efficient instruments of more than two feet aperture could be made, the attention of Congress was called to the deficiency in the National Observatory at Washington, which contained nothing better than an ancient Munich refractor of 9½ inches. In 1870 a bill was passed by Congress authorizing the superintendent to contract for a telescope of the largest size of American manufacture. The aperture agreed on was twenty-six inches, and the contract was awarded the Clarks. It proved difficult to obtain glass of this size, and it was a year before the rough disks could be had from Paris. The glass arrived in December, 1871; the polishing was completed in October, 1872. The whole instrument was ready for observation in November, 1873. The Clarks received $46,000 in gold for it. Some of the other glasses constructed by the firm were the 12-inch for the Victoria University, 12½-inch for Morrison, 15½-inch for Wisconsin, 16-inch for Warren Observatory, 18½-inch for North-west University, 20-inch for Denver, 23-inch for Princeton, 26-inch for Virginia, 30-inch for Russia, and 36-inch for the great Lick Observatory of California. This last lens was at the time the largest in the world.

After the Lick the next great telescope to come from the workshop of Alvan G. Clark was one ordered many years ago by Charles T. Yerkes, of Chicago, for the University of Chicago.

This was originally intended for the University of Southern California. When the great Lick telescope was set up the people of Southern California immediately resolved not to be outdone, and determined to place a still larger one on the summit of Wilson's Peak, near Los Angeles. The boom in that section of the country was then at its height, and the money was quickly subscribed. The glass disk was obtained from Paris, and to the Clarks was given the order for the lenses. But meantime the boom died out, and the subscriptions were not paid. Prof. Hale, of the

Kenwood Observatory, of the University of Chicago, recommended Mr. Yerkes, a patron of the university, to purchase the disk.

The objective lens for this telescope is 40 inches in diameter and 3·6 through at the thickest part. Its cost was $100,000. It was the largest undertaking of the kind ever known, and Mr. Clark worried greatly over it. When it was delivered he said he felt as if he had been relieved of a crushing load. The conveyance of the lens to the observatory was to him a source of much anxiety.

The lens was wrapped in flannel and cotton batting and then packed in a box padded with curled hair. This box was fixed on springs and the precious freight was shipped in a parlour car. Less than ten days ago Mr. Clark returned from the trip West, and to some of his friends he said he intended to retire from business.

He was the only surviving member of the famous firm. Three married daughters survive him, his son, who was expected to carry on the famous work, having died in childhood.

ALEXANDER FREEMAN.—The Rev. Alex. Freeman, Rector of Murston, near Sittingbourne, died on June 12 last from heart disease, after suffering from bad health for four years past. Mr. Freeman was born on 1838, January 28, and after being educated at Merchant Taylors' School entered at St. John's College, Cambridge, in 1857, of which foundation he was elected a Scholar. He was Fifth Wrangler in 1861, and in February, 1862, won the Chancellor's Gold Medal for legal studies. He was elected Fellow of his College in 1862 May, and took his M.A. degree in 1864. Though Mr. Freeman's chief work in life was associated with the Church, he did much astronomical and scientific work of a high order. He was Examiner for the Mathematical Tripos in 1874 and 1875; he acted as deputy for the Plumian Professor of Astronomy at Cambridge from 1880 to 1882; he translated and compiled an English edition of Fourier's 'Mathematical Theory of Heat,' and added to it many valuable original notes; and recently brought out a new edition of Cheyne's 'Planetary Theory.' To the 'Monthly Notices' of the Royal Astronomical Society, of which he was elected a Fellow in 1864, he contributed various papers on different subjects, and he has also made contributions to this magazine. He was for some time the Director of the Saturn Section of the British Astronomical Association; but had to give up this work on account of failing health. It may be added, as evidence of his attainments, that on the resignation of the late Mr. Stone from the Directorship of the Cape Observatory, Mr. Freeman was spoken of as not unlikely to be his successor.

Mr. Freeman has been Rector of Murston since 1882, and was also Rural Dean of Sittingbourne. He leaves a widow and four young children.

ARMINIO NOBILE.—We are also informed of the death of Prof. A. Nobile, Second Astronomer in the Royal Observatory of

Capodimonte, Naples. Besides in the work necessary to his official position, Prof. Nobile chiefly occupied himself in astronomy as applied to geodesy, and has published many papers on determinations of longitude and latitude. He was Professor of Geodesy in the University of Naples. He has also made researches on double and multiple star systems, published chiefly in the 'Transactions' of the Naples Academy and in the 'Astronomische Nachricten,' where will also be found papers by him on instrumental adjustments and general practical astronomy.

Prof. Nobile died on 1897 June 12, aged 59.

EARLY COMETARY KNOWLEDGE.—M. Gustave Ravené sends us the following from Paris:—"Searching in the National Library for some ancient mathematical facts, I had occasion to refer to the 'Mishnab,' and consulted the Babylonian Talmud. Reading more than my immediate purpose required, I came upon a passage where the Rabbi Josua (1st century A.D.) spoke of a star which appeared every 70 years. In a letter by Rapoport, which forms the preface to a Hebrew work by Stominsky, the statement is made that Rabbi Josua calculated the return of Halley's comet.

"Of course the fact of the calculation is out of the question, but it is not improbable that the periodicity was recognized, for the following reasons:—

"Halley's comet appeared in B.C. 12, the Herodian date noted for the rebuilding of the Temple.

"The appearance of A.D. 66 was contemporaneous with the beginning of the Jewish war and the destruction of Jerusalem. This must be the appearance which was noted by the Rabbi Josua.

"The chief compiler and editor of the Mishnab was born about A.D. 140, and an appearance of Halley's comet in the year A.D. 141 has been established without doubt. Likewise it must have appeared in 215–217 A.D.

"Then it is not impossible that Seneca's remark concerning the path of comets was known to any of the great Jewish Rabbis, and that they noted the regularity of the four above appearances. This is only of interest as a curiosity. The fact of periodicity being recognized does not give any claim to great astronomical knowledge."

VARIATION OF LATITUDE.—Dr. Chandler's ingenious theory of the motion of the Earth's axis of rotation within the Earth itself, while not yet accepted with absolute certainty, is nevertheless annually receiving additional support from observational evidence. It will be remembered that Dr. Chandler asserts that the north pole of the Earth moves round the circumference of a long and narrow ellipse in 365 days. The axes of the ellipse are respectively $0''\cdot30$ and $0''\cdot08$, and the major axis lies along a meridian 45° east of the Greenwich meridian. Combined with this the pole moves in a circle of radius $0''\cdot12$ in a period of 427 days. The combination of these two motions, which sometimes act in the same direction and at other times oppose one another, causes the pole to move in a complicated curve, as may be seen by reference to our number for 1895 March, where was reproduced a diagram by Dr. Chandler showing the motion of the pole deduced from observations during the period 1890–3, with a continuation predicting the motion up to the middle of 1895. It is obvious that a comparison of the observed position of the pole during the eighteen months from 1894·0 to 1895·5 with this prediction will be valuable evidence for or against the truth of the hypothesis. In a recent number of the 'Astronomical Journal' (no. 402), Dr. Chandler has an article on the subject in which he says that comparisons of this kind that he has made have confirmed his previous deductions as to the nature of this motion, and have not furnished any material improvement of the numerical elements, and he gives an illustration. Taking as data for discussion observations made at Kasan, Potsdam, Karlsruhe, and Strassburg in Europe, and Bethlehem and Columbia in America, he, assuming that his previously determined value of the 427-day circular motion is correct, deduces the elements of the annual elliptic motion. In the following table the first three lines give elements previously published deduced from observations; the last two lines give elements deduced from the data now discussed:—

T.	*ω*.	*a*.	*b*.
	°	"	"
1890 April 11	44·7	0·309	0·112
1892 April 1	51·4	0·311	0·054
1893 March 27....	23·4	0·289	0·082
1894 March 27....	58·1	0·244	0·141
1895 April 4	34·6	0·368	0·046

Where T is the date when the pole of figure is at its maximum distance from the pole of rotation, *i. e.* when it is at the end of the major axis, *a* and *b* the major and minor axes, and *ω* the direction of the major axis.

JUBILEE HONOURS.—In the distribution of honours on the recent historic occasion astronomers have had their share. The Astronomer Royal has been created a Companion of the Bath; Dr. Huggins and Prof. Lockyer are now Knights Commanders of the same Order; and a representative of navigational astronomy in the person of Admiral Wharton, H.M. Hydrographer, is also now entitled to add K.C.B. to his name. May we offer our congratulations to all these distinguished gentlemen?

AT the Royal Society Conversazione on June 16, the function where the costumes of the men for once rivalled those of the ladies in brilliancy, the astronomical exhibits did not by any means receive the least attention. One of the three lectures given was by Prof. Lockyer, who described the arrangements made for observing the Solar Eclipse of August 1896, with a mechanical lantern-slide, showing the different phenomena of the eclipse and the appearance of the corona as it would have been seen if only the clouds had allowed at Varanger Fiord. The photographs of the Moon taken with the Thompson telescope, exhibited by the Astronomer Royal, also came in for a large share of attention, and some pictures from the Paris 'Photographic Atlas of the Moon' were also shown.

The Lick Observatory.—It is generally well to allow a considerable margin for exaggeration in newspaper accounts of astronomical matters, so we write this note without expressing any opinion as to the accuracy of the information. We have had sent us a copy of the 'San Francisco Chronicle,' dated 1897, May 23, which contains a racy article, the gist of which is that the 3-foot reflector which Mr. Crossley, of Halifax, presented to the Lick Observatory in May 1895, has turned out to be rather a white elephant. The newspaper prefers to call it a "bone of contention." The facts seem to be that the Director, Prof. Holden, thinks this instrument should be used, and has instructed one of his assistants to use it. The Assistant, on the other hand, does not feel inclined to tackle what he considers an antiquated and difficult to manage instrument and to neglect work which is more congenial to him. The Assistant is supported by his colleagues in this view, and most of these gentlemen have taken the opportunity each to air his own personal grievance in the columns of the newspaper. So there is civil war on the top of Mount Hamilton, and the Regents of the Observatory have had to make an enquiry.

The Yerkes Observatory.—We hear from Prof. Wadsworth of a very unfortunate accident at the Yerkes Observatory. Only a few days after the 40-inch lens had been put in place, one side of the rising-floor broke loose from its supports and fell to the ground, being completely wrecked. Fortunately this happened when no one was on the floor, otherwise some lives would certainly have been lost, as the floor when it fell was at its highest position, nearly 50 feet from the ground. This, to the great disappointment of all the staff, will, it is thought, prevent the large telescope being used for some time, although the repairs to the floor will be pushed on as rapidly as possible.

Double-star Measures.—Recently published observations of double stars will be found in 'Ast. Jour.,' No. 397. Prof. H. S. Pritchett, whom we are glad to know is again at work with the micrometer, contributes a series of measures made with the 12½-inch telescope of the Morrison Observatory (Glasgow, Missouri) during the summer of 1896. There is also a series in the same number made by Prof. W. J. Hussey at Lick, some with the 12-inch and some with the 36-inch.

A telegram from Mr. Douglass, of the Lowell Observatory, dated May 27, stated that "the third satellite of Jupiter rotates in $7^{d}\ 5^{h}{\cdot}1 \pm 1^{h}{\cdot}2$." This is nearly the same as the length of its period of rotation about the planet. It has been announced later that Mr. Douglass comes to a similar conclusion as to the fourth satellite. It may be remembered that the suggestion that the periods of rotation and of revolution of these satellites are the same is said to have been first made by Sir W. Herschel, and resulted from observation of variation of their brightness when in different parts of their orbit.

A CURIOUS paragraph about an observation of the Moon has been going the round of the papers. Somebody in Worcestershire said that on the night of Tuesday, June 15, at 9.30, he saw across the Moon three black bars like boards, and also that there was seen a projecting ball of light, something like a crown. A commentator on this makes the facetious but obvious suggestion that the Jubilee is being celebrated in our satellite, and that the three boards are part of a stand for spectators.

ASTRONOMERS will be interested to know that Mr. Henry Frowde has recently published 'Tables for facilitating the Computation of Star-Constants,' by the late Mr. E. J. Stone, M.A., F.R.S., modified and revised by H. H. Turner, Savilian Professor of Astronomy in the University of Oxford. Prof. Turner claims to effect a considerable saving of time by the modifications he has introduced.

THERE will be an Annular Eclipse of the Sun on July 29, of but little interest. The line of central eclipse lies almost wholly in the Ocean.

THE portrait of the late Mr. Stone, which forms the Frontispiece to this number, is from a photograph by Messrs. Maull and Fox.

FROM AN OXFORD NOTE-BOOK.

PROBABLY no thrilling event has less connexion with Astronomy than the celebration of the 60th year of Her Majesty's reign, commonly called the Diamond Jubilee. Various devices for introducing it into these notes with an air of legitimacy have presented themselves, only to be dismissed as hopeless. I even took some scribbling paper with me on June 22, in the hope that the Royal Procession itself might inspire,—by anticipation perhaps: but nothing came of it. It only remains to say, *à propos* of nothing at all, that the whole affair was successful beyond belief, for although I was there myself and saw it, I am still troubled with curious vague doubts whether everything *could* have passed off so superbly as it seemed. The very completeness of the success involved a certain lack of incident, which becomes apparent on scanning the columns of this morning's paper. Doubtless it is better so; and, taking the hint, we may pass on to other matters. But such events, bringing others in their train as they will, are not conducive to orderly arrangement of thought: and I must beg for some consideration this month.

BUT Astronomy and the Jubilee are not altogether dissociated; though we may trust that the honours bestowed on the Astronomer Royal, on Sir W. Huggins, and on Sir J. N. Lockyer are due more to merit than to occasion. I would venture humbly to join in the congratulations of the editors elsewhere expressed; and to call attention to the utterance of the 'Standard,' which on the eventful morning remarked, "The Companionship of the Bath, which falls to Mr. W. H. M. Christie, the Astronomer Royal, is no doubt an earnest of higher honours in the future."

REJOICING in England has been nearly simultaneous with weeping in India. I have no accounts of the terrible earthquake beyond those furnished by the daily papers, but the story they tell is serious enough. There also seems to have been a severe shock of earthquake in South Australia on May 11. From a newspaper kindly sent me by Sir Charles Todd, it is to be gathered that "every one was much more frightened than hurt"; but considerable damage was done to buildings. Perhaps the following extracts may interest those who have not seen Australian papers :—

"South Australia experienced a considerable shaking-up on Monday afternoon, when the most severe earthshock ever known in the colony upset the equilibrium not only of the people but of the houses from one end of the colony to the other. It was just twenty-six minutes past two when the earth began to move, and instantly there was the wildest excitement in the city. Buildings developed a swinging motion, chimneys rocked to and fro, bells rang, chandeliers rattled, the pavements shivered, and people fled into the streets in a state of wild alarm. The sensation was felt for fully two minutes, and not a few superstitious folk thought the end of the world had come."

"The Government Offices, where some hundreds of men are employed, presented an unwonted spectacle a few seconds after the earthquake passed over. When the tremor began the electric bells with which Ministers and heads of departments communicate with their subordinates were set ringing incontinently as though an impatient Premier were pressing each one of them, but for once the summons to the presence of authority was disregarded. The clerks really got out more quickly than they do at half-past four every day, when their alotted day's work is done. They had some reason to be alarmed, especially in the old Government Offices, for plaster was shaken down from several parts of the ancient walls, and in one room at the top of the building a mass of documents neatly bound in the insignia of the service—red tape—came to the floor with a crash. Some of the Civil servants who made their way to the pretty quadrangle were in time to see the water in the large central fountain tossing as though a hurricane had swept over its surface, which is never ruffled by the winds that blow outside the Government Offices."

BUT the last month has brought news of a mishap which concerns astronomy more nearly than an earthquake. Whatever may be the merits of rising-floors, they do not seem to include absolute safety for the observer. A letter from the Yerkes Observatory tells of an accident which was fortunately unattended with personal injuries, but might have been a disaster of the first magnitude. The rising-floor fell from nearly the top of its run (45 feet) and smashed itself badly, so that it will probably be nearly a year before work with the telescope can be again commenced. That there was no one on the floor and even that the great telescope itself was uninjured, are things to be devoutly thankful for. I have not heard details and do not know what may be implied by "Barnard and Ellerman just escaping," but one does not like to think of the possibilities. The occasion seems to demand at least one scape-goat, and it is to be hoped for the sake of the future that the fullest details will be made public.

WITH regard to the note last month concerning the "adaptation of formulæ to logarithmic computation," I am happy to have received from such an experienced computer as Mr. W. Ellis the following note:—

"I was much interested in reading the note in the June number of the Observatory, p. 259, calling in question the supposed saving of labour by the use of adaptations of formulæ to logarithmic computation. In trigonometrical calculations in which I have been free to employ such methods as I thought best, I have frequently, in years long past, used the fundamental formulæ in preference to employment of the artifice of an auxiliary angle. As is pointed out in the case cited, and as I have on numerous occasions experienced, the real saving in adopting the latter course is doubtful, besides which there seems to be a greater simplicity in the fundamental formulæ. I confess that I never had the courage to suggest to others that they should follow my example; it seemed too awful a thing to presume to say that the accepted methods of text-books had not always the advantages that appeared to be claimed for them."

THERE is another row on Mount Hamilton. The case is still *sub judice*; or, at least, no decision has reached us on this side of the Atlantic, and comments on the merits of the case would be accordingly ill-timed. But it seems to be no secret that the disturbance centres round the 3-foot reflector which Dr. Common used to such good purpose as to get the Gold Medal in 1884; which was then acquired by Mr. Crossley of Halifax, who covered it with a beautiful dome; and which was presented by him a year or two ago to the Lick observatory, where it has become a bone of contention. The Director wants it used, and "the other astronomers—and on this the seven are a unit—declare that it is worth only $2500, is as antiquated in its mounting as Noah's ark, and has never proved its value to science. It is said that only one photograph has ever been taken with it, and that away back in the seventies."

Is it wrong to reprint such remarks in a scientific magazine? When read in the *San Francisco Chronicle* they can be properly discounted; but for setting them down here, there should be a special reason. I hope the one I advance may be found sufficient. I have seen something of the subsequent history of a good many instruments accounted first rate in their time, and it is melancholy enough to enlist our sympathies. Why should the Derby-winner end his days between the shafts of a four-wheeler? Are we modern astronomers entirely devoid of sentiment? No doubt we are going ahead pretty fast just now, but record-breaking is a poor pursuit if we have no respect for the records broken. We can see how it may be said of the telescope which in 1883 revealed nebular structure photographically to the delighted astronomical world—how it may be said in a moment of irritation in 1897 that "only one photograph has ever been taken with it, and that away back in the seventies"; and we can even find excuses for a man "entirely inexperienced with the instrument," who "gave it as his opinion that the mounting of the reflector would have to be changed before it could be used"; but we cannot but sincerely regret that the instrument has been subjected to such unsympathetic criticism. When Lassell had done his work with his big reflector he broke it up. Dr. Common has often told me this, and I am pretty sure he wishes he had done the same; otherwise I should not dare to write so explicitly. But the opportunity for preaching is too valuable to be lost. One is pretty certain to get oneself thoroughly disliked by half-a-dozen people; but then there is the chance that by speaking out one may save some epoch-making instrument from ignominy. I think it is worth risking.

SOME weeks ago Mr. McClean slipped away from the Council meetings of the R. Ast. Soc. with a brief though most courteous apology; and probably very few know where he is at this moment. As a matter of fact he is at the Cape Observatory. He has been for the last year or two photographing the spectra of the bright stars in the northern hemisphere with a magnificent objective-prism placed in front of a 13-inch telescope of the Astrographic Chart pattern at his house in Tunbridge Wells, and by this spring had completed all down to the 3rd magnitude. The large angle of the prism gave spectra of a beauty hitherto unequalled. During his visit to England last year Dr. Gill invited Mr. McClean to complete the survey of the whole sky by taking the prism to the Cape and placing it in front of the existing Astrographic instrument; and the invitation was gratefully accepted. Mr. McClean left England on May 1, and his many friends will be glad to hear that he has arrived safely and is hard at work. He finds the sky better even than he hoped, and had 25 photographs of a dozen stars in the first fortnight. We may join heartily in wishes, not only for the success of his work, but that he may see the arrival of the fine instrument which he has presented to the Cape Observatory.

MAY we tender our best wishes to Prof. Foerster of Berlin, on the occasion of the approaching marriage of his daughter Hulda with Dr. S. Heckscher?

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 256. AUGUST. 1897.

THE BRITISH ASTRONOMICAL ASSOCIATION.

The concluding meeting of the current session of the British Astronomical Association was held at Essex Hall, Essex Street, Strand, on Wednesday, June 30, *Mr. N. E. Green*, *F.R.A.S.*, President, in the Chair.

The election of five new Members of the Association was confirmed, and thirteen Candidates for Membership were passed for suspension.

Mr. E. W. Maunder read a paper on the "Umbral Protrusion of Sun-spots," by *Mr. F. K. McDonall*, of the New South Wales Branch. The writer stated that during the past three years 153 spots exhibiting the phenomenon of umbral protrusion had come under his notice, and these he had divided into various classes or groups in which the umbræ had presented some similarity of appearance. Of the 153 spots, 108 were observed at ingress and 48 at egress. The entering spots presented the appearance in a much more clearly-defined manner than the others. The umbral protrusion was also observable in spots in all latitudes, and there appeared to be a subsidence or falling in of the umbræ previous to the disintegration of a spot. The conclusion that Mr. McDonall had arrived at was that the phenomenon was, in some cases at any rate, objective.

Capt. Noble said that the paper just read had a peculiar interest for him, because it seemed to arrive, by an altogether different mode of procedure, at the same result as his friend Mr. Jenkinson, who had been working in the same direction for a long period, had reached. He thought that Mr. Jenkinson's drawings were, without exaggeration, the most beautiful he had ever seen, and he (Mr. Jenkinson) had arrived at the conclusion that the form of a sun-spot in section was that of a lunar crater.

In reply to Miss Brown, *Mr. Maunder* said he believed that Mr. McDonall observed by direct vision.

Capt. Steele remarked that he looked upon sun-spots as purely atmospheric phenomena, being somewhat similar to the typhoons and cyclones of the tropics. He wished to know whether sunspots had ever been observed in high latitudes.

Mr. Maunder said he should agree perfectly with what Capt. Noble said was Mr. Jenkinson's idea of a sun-spot, if only it were put on a different scale. He supposed that the average depth of a sun-spot would be, perhaps, from one-hundredth to one-thousandth of its diameter, whilst a lunar crater would be from one-tenth to one-thirtieth. But when allowance was made for this difference in scale, the average form of the two did, he thought, show a resemblance; in the centre there was a convexity of the bottom of the sun-spot, which might in certain cases rise above the level of the ordinary photosphere. The work upon which he had been engaged at Greenwich, the photographic record of sun-spots, was not one which brought out the feature of umbral protrusion at all well, because near the edge of the Sun the photographic intensity fell off very considerably, and as the exposures were necessarily regulated so as to give the best general result, spots near the limb were apt to be poorly defined. Mr. McDonall's paper was, so far as he was aware, quite a fresh contribution to the literature on the subject, and they had therefore every reason to be grateful to him for bringing it forward.

Mr. Petrie read a paper, contributed to the Jupiter Section by *Mr. C. F. Smith* of the Edinburgh Branch, on "The Appearance of Jupiter during the 1896 and 1897 Apparition." Mr. Smith said that Jupiter's disc had presented an exceedingly interesting aspect, and changes appeared to have been rapid and considerable. The South Equatorial Belt had been strong and broad, the edge bordering the Equatorial Zone showing many light and dark markings and occasionally faint wisps. The Belt has shown the usual brown colour, sometimes having a reddish tinge. The shoulder following the Great Red Spot had been dark and well marked. The Red Spot had not been seen, its place being marked by a very light colour.

Mr. J. D. Hardy suggested that the Red Spot might be somewhat similar to our Sahara Desert. That desert was a dry region with a cloudy region all round it, and probably, viewed from Venus, would have somewhat the same appearance as the Red Spot as seen from the Earth.

Lord Hampton said that it had been stated in the Journal that the Red Spot had been seen of a brick-red colour by an observer in Australia during the same period.

Capt. Noble remarked that the suggestion as to the Red Spot being a desert, presupposed that the surface of Jupiter itself is seen, which he did not for a moment believe. If Jupiter were not red hot, it was at any rate a great deal hotter than any place he would like to live in. He had seen the shadow of one of the satellites as a bright chocolate colour, which he took to indicate that the planet was to some extent self-luminous.

Mr. Newbegin said it must not be forgotten that the Red Spot had been visible for something like twenty years.

Mr. Maunder added that the Red Spot came under observation in 1877 or 1878, but there was good reason for thinking that it was an old form that had appeared again, and that it had been seen by Cassini some 200 years ago.

Mr. Holmes said that it was an entire assumption that Jupiter was red hot.

Mr. E. R. Hicks asked whether a satellite or a shadow of a satellite had been observed to pass over the Red Spot.

Mr. Davis said he had seen and sketched both Satellite III. and its shadow on the Red Spot. He thought there was evidence that the red spots were connected with the more permanent matter of the planet.

Mr. Hardy said he did not intend to imply that the spot was a desert, but simply that it was a cloudless region with a cloudy region round it.

The *President* remarked that a great variety of colour was one of the first things that a keen eye for colour would notice in making drawings of Jupiter. As a result of many years' observation of Jupiter, he thought there was very strong evidence for thinking that the general body of the planet when free from surface markings of a white nature was of a warmish tint.

Mr. Corder said he had records of observations of about the period referred to by the President when the Equatorial Belt of Jupiter was exceedingly red.

Mr. Petrie read a note by *Mr. MacEwan* on "The Canals of Venus." He had forwarded a tracing of Mr. Lowell's latest chart of Venus to Mr. Foulkes at Malta, and that gentleman's reply was a complete confirmation of Mr. Griffiths's recent observations.

Mr. Griffiths sketched on the blackboard a marking recently observed by him, which had been confirmed independently by Rev. T. Phillips. In reply to Capt. Noble he said that he had never been able to detect the slightest change in the markings which might be due to rotation of the planet.

Capt. Noble said he was driving at the question of rotation, because if Mr. Holmes had been correctly reported in the Journal, he appeared to think that both the people who believed in the long period and those who believed in the short period of rotation were right.

Mr. Thwaites mentioned that some time since he was observing Venus together with Mr. E. W. Maunder and Mr. Newbegin, and they had all three seen a marking in nearly the same position as that shown by Mr. Griffiths, but it was not nearly so distinct and was very diffused.

Mr. Maunder said he could quite confirm what Mr. Thwaites had said.

Mr. Petrie announced that Mr. Tyson Crawford ("Dollond") had promised a 2-inch transit instrument for the proposed Observatory in the gardens of the Royal Botanical Society.

The Association then adjourned until the last Wednesday in October.

The Great Meteoric Shower of November.

[Continued from p. 277.]

THE heights of the meteors range from about 90 to 50 miles, but these limits are sometimes exceeded. Prof. Newton concluded that the average heights of the Leonids are greater than those of the Perseids, for he found values of 96·2 to 60·8 miles for the former, and 69·9 to 56·0 miles for the latter. Prof. Newcomb in 1867 determined the mean height of 9 Leonids as from 102 to 47 miles. The writer at Bristol found a mean height of 85 to 54 miles for 6 Leonids, and of 81 to 51 miles for 15 Perseids. Of the 6 Leonids 5 were observed in 1896 as follows:—

Date.	G.M.T. h m	Mag.	Height at first. miles.	Height at end. miles.	Path. miles.	Velocity. miles.	Radiant. °	°	Places of observation.
1896 Nov. 14...	13 54	>1	95	51	85	63	150	+22	Slough and Cardiff.
14...	14 1½	♃	85	49	70	46	148	+20	Slough and Cardiff.
14...	14 29	1−♂	92	55	60	...	150	+23	Slough and Bridgwater.
14...	17 27	1−♃	100	71	33	33	149	+24	Bristol and Bridgwater.
14...	17 45	3	75	50	29	58	150	+24	Slough, Bristol, and Bridgwater.

From the large number of brilliant meteors doubly observed in 1866 it may be interesting to quote a few examples:—

	h m	
1866 Nov. 13...	11 22	Mean height 60 miles above the surface of the earth.
	12 40¾	Passed over St. Andrews in Scotland. The meteor consisted of 3 fragments, each as bright as Venus, and left a streak visible for 8 minutes. The mean height was about 50 miles.
	13 8	Fell from 100 miles above Petersfield to 53 miles above Tiverton, with a flight of 120 miles. The streak was 17 miles in length, and it remained visible in a contracted form for 10 minutes.
	14 41	One of the most brilliant meteors of the display. It left a streak at an altitude of 64 miles above the neighbourhood of Dundee.

Prof. Newton investigated the real positions of several conspicuous meteors and their streaks observed in 1868. He was led to conclude, from the motion of the streaks and their curious distortions, that a northward current of air prevailed below an elevation of 54 miles, and that between this and a height of 60 miles an air-current existed in motion towards the south, while above this again there was a third current moving in a northerly direction. The double appearance of the streaks observed with a telescope led Prof. Newton to conjecture that the meteors in such cases were really double. A better explanation may be, however, that these streaks were luminous cylinders formed by the offrush of exhausted material from the meteors during their combustion, for it is evident that such a process would give an apparently duplex form to the streaks as observed.

Minor Showers contemporary with the Leonids.

These are very numerous and always make their presence felt during watches for the chief display. This is especially the case when the Leonids are only weakly represented. When an endeavour is being made to determine the horary number of the latter it is desirable to consider the minor radiants and to separate the true Leonids from the streak-leaving meteors belonging to other radiants in the same region of the sky. Many observers count every meteor proceeding from the general direction of Leo as necessarily a Leonid, and thus the horary rate is exaggerated. If an inexperienced observer gives 20 as the total number of Leonids seen in an hour, the fair inference is that not more than 15 or 16 were true Leonids. During strong returns of the shower this point need not be considered, for the minor streams can then exercise very little influence on the results and are relatively obliterated by the great superabundance of Leonids.

It is remarkable that in November a large number of meteors diverge from radiants in Taurus, Perseus, and the surrounding region. In fact during an ordinary year, when the Leonids are not very numerous, there are considerably more Taurids and Perseids than Leonids. Heis at Aix-la-Chapelle found from his observations in November, during the years 1839 to 1847, that out of 407 meteors registered, 171 proceeded from Perseus near the star η, while only 83 were directed from Leo. In 1851 Schmidt, of Bonn, remarked that Perseus appeared to furnish the greater number of meteors not only in August, but throughout the year. In order to examine this conclusion, the writer dissected his own observations of meteors during the years from 1876 to 1887 in November and found that amongst an aggregate of 1202 paths recorded there were 156 Taurids, 78 Perseids, and 88 Leonids. The predominance of Taurids is very marked and would appear to show that they have developed greater strength than formerly and supplanted the numerous autumnal Perseids seen half a century ago by Heis and Schmidt, who were the early and able pioneers of systematic observation in this department of astronomy.

The Taurids and Perseids, so abundantly in evidence during the month of November, are never liable to be mistaken for Leonids for several reasons. Their radiants are situated in a distant quarter of the sky, and the meteors move slowly, often leaving trains, but rarely, if ever, displaying the phosphorescent streaks of the Leonids. Besides the Taurids and Perseids referred to, there are many other radiants visible in the southern and western sky in the morning hours at the middle of November. Andromeda, Cassiopeia, Aries, Cetus, Camelopardus, and Auriga all furnish their quota of feeble meteoric showers. But it is with the radiants in the same quarter as Leo that the observer is chiefly concerned. These are very likely to, and often do, create confusion, for the meteors present a similarity of aspect to the

Leonids, and their directions of flight are often pretty accordant with them. There are showers near δ and τ Cancri, ι, θ, μ, ψ, ξ, ε, and ζ Ursæ Majoris, ζ and λ Hydræ, and χ, π, τ, and β Leonis, with many others outlying. These cannot be regarded as detached or branch streams of Leonids, for they exhibit a precision of radiation, and are sufficiently far removed from the Sickle, to warrant their being considered as distinct and separate systems. That so many radiants are clustered in this region of the sky is not at all remarkable, for we shall find the eastern quarter equally rich in showers if it is watched in the mornings of August, September, or October. The radiants surrounding the Leonid centre are therefore to be considered as an accidental grouping of positions, since it is not by any means special, but a circumstance naturally following upon the very plentiful distribution of showers all over the sky and particularly in that region near the apex of the Earth's way. The latter is not, however, always the most prolific, for there are many prominent systems which acquire their greatest activity at positions far to the west of that part of the ecliptic towards which the Earth's motion is directed.

It may be useful to give a table of 72 of the minor radiants usually visible at the epoch of the Leonids and deduced from observations at Bristol. Many other feeble showers exist at this period in addition to those included in the table. It is believed that the great majority of these contemporary radiants are manifested every year, though their richness in individual cases is liable to variation.

Radiant-points of Meteoric Showers, November 10–15.

°	°	
5	+52	S.
8	+36	M.
19	+30	S.
20	+ 8	S.
27	+71	M.T.
29	+37	S.T.
30	+16	R.
32	+51	M.
43	+ 6	S.
45	+60	M.
46	+21	S.T.
48	+43	S.
50	+30	S.
53	+71	M.
57	+18	S.
60	+28	S.T.
60	+49	R.
62	+34	R.
63	+21	M.T.
64	+11	S.T.
70	+66	M.
72	+ 4	R.
73	+42	R.
75	+15	M.
77	+32	M.T.
79	+56	R.
80	+24	S.
86	+75	M.
87	+20	M.
97	+29	R.
99	+44	R.
102	+73	R.
103	+48	R.
107	+11	R.
107	− 3	R.K.
110	+25	R.
115	+84	S.
120	+15	R.K.
121	± 0	R.K.
124	+55	R.K.
*125	+40	R.K.
*132	+21	R.K.
*133	+31	R.K.
133	+48	R.K.
133	+70	R.
*136	+ 8	R.K.
*141	+27	R.K.
143	+50	R.K.
143	+69	R.
*146	+ 8	R.K.
*148	−12	R.K.
*154	+12	R.K.
*154	+41	R.K.
*157	+49	R.K.
157	+74	R.
*166	+ 3	R.K.
*166	+31	R.K.
170	+47	R.
177	+10	R.K.
178	+34	R.K.
184	+25	R.K.
190	+21	R.K.
191	+58	R.
194	+67	R.K.
200	+57	S.
213	+35	S.
213	+75	S.
238	+49	S.
262	+64	S.
277	+78	S.
284	+43	S.
315	+60	R.
338	+59	R.

R., Rapid; S., slow; M., moderate speed; K., streaks; T., trains. The radiants marked with an asterisk signify those supplying meteors which are very liable to be mistaken for Leonids.

The visibility of many of the showers given in the list is not confined to a few nights at the middle of November. Before the advent and after the extinction of the Leonid shower a large proportion of the minor systems appear to be in distinct operation. For the most part, however, they are extremely feeble and will certainly escape observation unless the sky is watched for long periods and the path-records of several nights combined for the purpose of deriving the radiant-points.

To the casual observer of meteors it cannot be said that the multitude of feeble showers possesses much significance. The great majority of the people who will watch the anticipated display of Leonids during the few ensuing years will do so for the mere gratification which such a rare spectacle must afford, and will gaze at it with much the same feelings as they regard a pyrotechnic display which terminates a fête. But everyone who witnesses the coming showers will be sure to observe a few "sporadic" meteors, as they used to be called, and these will arrest special attention from the fact that they present a great dissimilarity of direction and speed to those of the main display. They will add a certain variety to the phenomenon if they do not much affect its brilliant appearance. To the regular meteoric observer the minor showers are, however, of intense interest, and they form a detail which cannot possibly be overlooked. The difficulty of ascertaining their radiants adds a zest to their pursuit, and many of them have been satisfactorily determined, but a large number of further observations are needed to enlarge the list and increase its accuracy.

Apart from the Leonid fireballs, common to the epoch Nov. 11–16, the period is a memorable one for large meteors. The latter belong to many different showers, though the Taurids undoubtedly claim a larger share than any other system. Below is a list of some of the fireballs seen at this time; it is by no means complete, but will sufficiently prove that the season is a remarkable one for the frequency of fireballs:—

		h m	
1634	Nov. 13...	......	A large ball of fire with a long tail seen between Joachimsthal and Gottesgabe, Bohemia.
1761	12...	......	A large fireball exploded with a prodigious noise and set fire to a house near Dijon.
1791	12...	6 39 A.M.	A large meteor resembling a rocket seen by Lichtenberg at Göttingen.
1799	12...	6 0 A.M.	A globe of fire appeared with a strong hissing noise and finally exploded.
1803	13...	8 30 P.M.	A very remarkable fireball seen in England.
1813	10...	6 40 P.M.	A large detonating meteor observed in England.
1818	13...	7 15 P.M.	A fine meteor diffusing a blue light and accompanied with sparks appeared in England.
1819	13...	7 0 P.M.	A very extraordinary globe of fire appeared at Haïti. Another seen in Bohemia on Nov. 14.

		h	m	
1820	12...	4	0 P.M.	A globe of fire, accompanied with thunder, was seen in the province of Kursk, Russia.
1822	11...		......	A globe of fire was seen at Freyberg. On Nov. 12 a large shooting-star at Potsdam, and on Nov. 15 a fireball at Apenrade.
1824	13...		......	A globe of fire observed at Mentz.
1825	14...	8	0 P.M.	An igneous meteor, which exploded, was observed in Scotland.
1835	13...		......	In the Department of L'Ain, France, an aerolite fell and set fire to two houses.
1836	11...		......	A stone fell at Macayo, Brazil. A piece weighing 6·4 grams is in British Museum.
1837	16...		......	Fireball observed in Pennsylvania, U.S.A.
1842	13...		......	At Pas de Calais a fireball appeared and gave a detonation.
1854	16...		......	A very large meteor seen at Hamburg.
1856	12...		......	A stone fell at Trenzano, Italy; a piece weighing 9·4 grams is in British Museum. On Nov. 15 a fireball seen at Troppau.
1858	12...		......	A large meteor seen at Oxford.
1859	15...	9	30 A.M.	Fireball appeared, as bright as the Sun, in the United States. It fell from a height of 100 to 8 miles. Radiant 235° +30°.
1860	15...		......	Fireball equal to the Sun seen in New Jersey. It caused a great smoke and detonation.
1861	12...	5	49 P.M.	Fireball about $\frac{2}{3}$ diameter of ☽ seen. Height 95 to 20 miles. Radiant 61° +21°.
1861	15...	10	15 P.M.	Fireball brighter than ♃ seen at Greenwich.
1863	11...		......	Large fireball seen at Bautzen. On Nov. 12, 8.46 P.M., a fireball seen in London, and estimated to be as large as a tennis-ball. On Nov. 12, at 1.48 A.M., a fireball equal to Venus seen at Birmingham.
1864	11...	5	35 P.M.	Fireball = $\frac{1}{2}$ ☽ seen in France. Height 97 to 60 miles. Radiant 80° +35°.
1865	13...	5	0 A.M.	Fireball = ☽ passed from 187° +25° to 160° +62° at Streatham. On Nov. 13, 5.42 P.M., a fireball appeared, 3 × ♀. Height 85 miles. Radiant 85° +25°. On Nov. 13, 10.50 P.M., fireball seen, 4 × ♀, leaving a streak.
1868	14...	9	57 P.M.	Fireball seen at York. 2° long and 10′ broad.
1869	14...	4	47 A.M.	A large fireball observed in Scotland. Radiant in Pegasus.
1870	13...	9	38 P.M.	Fine meteor exceeding ♃ seen at Greenwich. Taurid.
1873	13...	7	48 P.M.	Fireball = $\frac{1}{2}$ ☽ seen in England. Height 57 to 18 miles. Radiant 84° +51°.
1879	11...	5	34 P.M.	Splendid meteor seen in Austria. Height 50 to 46 miles. Radiant 271° +11°.
1893	12...	6	53 P.M.	Meteor = ♃ at Bristol and Leeds. Height 66 to 36 miles. Radiant 40° +6°. Two other fine meteors (Taurids) seen at 8.40 and 10.9 same night.
1895	13...	11	15 P.M.	Fine meteor, 2 × ♀, with streak, seen at Dewsbury. Taurid.
1896	12...	11	8 P.M.	Brilliant meteor seen in England. Height 52 to 21 miles. Radiant 45° +4°.

The list might be considerably amplified by a thorough search through scientific journals; but the instances already quoted sufficiently demonstrate the period to be a special one as regards the abundant occurrence of fireballs. At some future period, when materials have accumulated, it will be useful to collect all the radiants determined at this season from doubly-observed fireballs and ordinary shooting-stars. Such a catalogue, if it brought out the conspicuous activity of showers like the Taurids, Perseids, and Aurigids in supplying large fireballs, would also give an indication of the vast number of radiants which crowd the sky near the close of the second week in November.

One feature of the Leonids which must at once strike the observer is their rapidity of motion. In this respect they offer a marked contrast to the slow steadily-descending meteors often seen at other times. The Leonids dart or shoot along with incredible speed, and generally complete their visible flights in less than one second of time. Unfortunately this element of duration is very difficult to determine in reference to objects of such transient character, so that the velocities computed from the roughly-estimated durations of flight exhibit great discordances and are manifestly affected by large probable errors. Accuracy can only be approximated by deriving a mean value from observations of several meteors; thus of 4 Leonids seen on the night following 1896 Nov. 14, the average velocity was 50 miles a second, which is undoubtedly rather in excess of the truth. Could we attain to greater precision in recording the visible durations, there seems little question that the observed speed would very nearly accord with the theoretical speed of 44 miles per second attributed to these meteors.

As to the real size of the individual Leonids, this varies to a considerable extent, and is certainly an unknown quantity. Some of the fireballs of this system must be tolerably large and compact bodies, though not nearly so large as their visible aspect would perhaps indicate, for the nucleus, divested of its outlying flame and glare would probably appear very small, and almost star-like in aspect, though of intense brilliancy. Prof. Herschel, in discussing some observations of the November meteors in 1865, remarked that "the illuminating power and, in consequence, the weight of the meteoric particles depend for their values directly upon the square of the distance of the meteors. The weight is at the same time inversely proportional, *cœteris paribus*, to the square of the velocity. The average weight of the November meteors is five grains." This deduction was from four meteors only. The weight of meteoric matter arrested at 30 miles per second speed was computed by the same authority for 20 Perseids seen in 1863 August 9 and 10. These weights vary from 28 grains to 7 lbs. 7 oz. 88 grs. The latter was for a meteor = ♃ in brilliancy, another = ♀ was found to weigh a little over 3 lbs. In estimating the apparent brightness of meteors observers often disagree, for at nearly same places one description compares the object with Venus, while another compares it with the Moon. Sir John Herschel considered the light of the full Moon as 6852 × Sirius, and 54,816 × Vega.

W. F. Denning.

[To be continued.]

A new Advance in Solar Physics *.

No fact about the Sun is better assured than the extreme rarity of his surroundings. The serrated scarlet chromosphere, the silvery structures of the corona, the vast equatorial effluences observed during total eclipses at sunspot *minima* make up but an "insubstantial pageant." They scarcely weigh in the balance; they all but evade gravity; their meshes are too fine to entangle even such ethereal invaders as comets. Their one appreciable faculty is that of shining. Yet, for purposes of investigation, this is just the one thing wanted, more especially when the light emitted is original and discontinuous. For the various rays into which it can then be separated by a refracting or diffracting apparatus are charged with meaning, both chemical and physical.

Now the luminosity of the chromosphere and prominences may be called exclusively their own. Moreover, it is of gaseous origin, and, therefore, characteristic. It is a bundle of significant bright lines. A few of these are constant, a multitude casual. The latter emerge eruptively, as the result of vehement surgings in a subjacent ocean of mixed metallic vapours, tossed up, as it were, in glowing spray into spectroscopic view. Essentially, nevertheless, the red solar envelope and prominences are alike composed of hydrogen, helium, "coronium," perhaps, and calcium. The importance of the last-named ingredient has only of late become fully known. And this for an obvious reason. It is represented by only one pair of lines—fine, bright reversals of Fraunhofer's broad, dark "H" and "K"; and they are situated close to the limit of ultra-violet visibility. Hence, the aid of photography was indispensable for their effective study. They were registered, it is true, as leading features of the chromospheric spectrum by Prof. Young in 1872; not without perplexity at what their presence seemed to imply. Indeed, the Dartmouth College astronomer refused to believe that the substance betokened was actually calcium, holding it impossible that a comparatively heavy metallic vapour should keep step with the upward excursions of hydrogen, the lightest of known gases, and of helium, evidently an analogous form of matter. He inclined rather to consider hydrogen itself responsible for the problematical rays. Sir J. Norman Lockyer, on the other hand, regarded them as emissions of a calcium-constituent, the terrestrial "element" being decomposed at the exalted temperature of the Sun. A remarkable series of spectral changes observed in the laboratory to accompany variations in the intensity of the electric current employed to produce luminosity was alleged in support of this view; above all, the progressive enfeeblement of the familiar "blue line" of calcium, concurrently with the enhancement of H and K, as molecular excitement was gradually worked up to the "hottest spark" stage.

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* From 'The Times,' 1897 July 10.

More and more clearly, as photographic methods developed, the calcium problem was perceived to be fundamental in solar physics. Plates exposed by Dr. Schuster to the eclipsed Sun at Sohag, in Egypt, May 17, 1882, testified to the surprising strength of the violet light emitted by its flame-garland; while, ten years later, Prof. Hale and M. Deslandres owed their conspicuous success in day-by-day prominence-portraiture to their choice of the K-line as the medium of their researches. It is, for several reasons, more convenient for use than its companion; but both possess the singular advantage of being naturally protected from atmospheric glare by the dusky sheathings encompassing them in the ordinary solar spectrum. By means of the double-slit photographs thus taken on K, fresh and most convincing evidence was collected of the extraordinary profusion in which calcium-vapour exists near the Sun. It proved to enter largely into the composition of the faculous appendages stretching, in curls and spirals, across the spot zones; it retained actinic power at the summits of the very tallest prominences, where even hydrogen was probably extinct. A limit is not easily set to the height at which it may continue to shine; 300,000 miles have almost certainly been transcended, while structures radiating H and K range quite commonly between 50,000 and 100,000 miles.

These startling facts must be considered in their relation to a special peculiarity. As already remarked, the twin-lines in the violet are, in the chromospheric emissions, isolated from all their usual associates. No trace remains of any other constituent of the calcium spectrum. Yet 75 of the dusky Fraunhofer rays in the general solar spectrum are due to the metallic base of lime. The question then naturally arises whether an identical kind of matter is really concerned in each case—whether the substance giving out H and K in the prominences is chemically indistinguishable from the metal volatilized in the reversing layer. A satisfactory answer could be furnished only by continued observation. Until calcium had been experimentally reduced to a "two-line" condition, discussions on the point were of slight avail.

This precisely is what Sir William and Lady Huggins have now achieved. In a paper read before the Royal Society, June 17 last, they describe a set of experiments showing, simply and conclusively, that calcium vapour, when sufficiently attenuated, radiates, under electrical stimulation, H and K alone. In other words, its spectrum corresponds exactly with that of the prominences. This final stage, at which the metal remained present only as a bare trace, was reached by successive approximations, the attendant spectral variations being photographically recorded. They form a connected sequence, obviously dependent upon the thinning-out of the emitting substance. Every other cause of change was, indeed, carefully excluded; and a spark of small intensity was employed throughout so as to keep the temperature uniform and moderate.

Thus the solar prominence-spectrum of calcium has at last been artificially produced; and the condition *sine qua non* of its production is unmistakably the extreme rarefaction of the emitting material—a condition amply fulfilled in the Sun. No dissociation-hypothesis is accordingly needed to explain spectral phenomena which have hitherto supplied the strongest argument in its favour. The difficulty, nevertheless, remains of accounting for the agility, so to speak, of the calcium molecule. Many times more massive than that of hydrogen, it yet seems to be endowed with a virtual lightness scarcely, if at all, inferior. The anomaly, to be sure, may be more apparent than real. Persistence of the H and K vibrations under circumstances which extinguish the glow of hydrogen is probably concerned in giving rise to the observed effects; while the seething agitations prevalent in chromospheric regions tend to counteract the sorting power of gravity. Neither in Sun or stars is the distribution of the various substances in their neighbourhood prescribed at all strictly by their several vapour-densities. The fiery envelopes of these vast globes obey no definite law of stratification. In the variable star Mira, for example, a layer of dense calcium is superposed upon a bed of ignited hydrogen, so as actually to obliterate by absorption a member of the harmonic series of its brilliant rays.

The discovery made by Sir William and Lady Huggins affords much-needed help towards the disentanglement of the parts respectively played by temperature and pressure in modifying spectra. The impossibility of arriving at satisfactory conclusions on the point has long constituted a stumbling-block in the way of astrophysicists. If not now wholly removed, it is at least pushed considerably to one side. The cleared path will doubtless be eagerly followed.

Some Reminiscences.

There are more readers of the 'Observatory' who, on receiving a new number, turn first to the papers "From an Oxford Note-Book" than would probably care to make confession of the fact. Not that this curiosity is to be deprecated when we consider the variety of information frequently to be found therein, and especially those marvels which from time to time, on occasions of scientific expeditions into flowery and western lands, are gathered together for our enlightenment. Truth, we know, is stranger than fiction, and for such increase of our knowledge we all alike feel correspondingly grateful. We have lately read in the May notes of a more homely matter—of a wonderful man, Chevalley, a Swiss, who is said to have acquired the art of keeping time like a clock, and who could indicate the quarters of an hour by his voice even when distracted by talking. To learn even this humble fact enlarges our views and shows that we ought not to consider Le Verrier to have been so very unreasonable in wishing to test the ability of a Greenwich observer to count seconds accurately through a minute of time out of hearing of the clock—which story I do not remember to have heard before, though probably I have, since the observer in question would be an old colleague of mine. Others have, however, unconsciously tried to emulate in some small degree M. Chevalley. Having myself, in my early days at Greenwich, had a good deal of practice as an observer of eye-and-ear transits, I used frequently, in the intervals between observations, amuse myself by testing how closely I could count 60 seconds away from the clock, which at that time I could usually do with little error, probably not altogether to the satisfaction of Le Verrier, not to mention M. Chevalley.

There was also another way in which the perception of the true length of a second became instilled into my inner consciousness. At one time, for many years, I had to pass some portion of every day in the chronometer room, in which the continued clang of a large number of chronometers produced a very distinct half-seconds wave of sound, and thus I came to practise the counting of a minute by half-second beats as well as by seconds. Of course the accuracy attained was not such as that one could distinguish between the different lengths of sidereal and solar minutes. Recently I again tried my ability accurately to count 60 seconds, without, however, reaching the same success as before.

The power of carrying on independently the counting of seconds was at times really useful. Occasionally at Greenwich, when the transit-clock required cleaning or repair, or when after the introduction of the chronographic system there was otherwise interruption, the clock temporarily placed in the transit-circle room would be one having a somewhat faint beat, and on a very windy night it sometimes happened that the beats could not be heard for a lesser or greater number of seconds. At the end of a transit, during which the clock-beats had been thus at times inaudible, it was a relief to find, if the object observed were the Moon, that the clock-counting had been correct, but I have no remembrance of there having been at such times any mischance. In observing transits of quick-moving objects it was in all cases my custom to take the second off the clock as the object approached the first wire, count through the transit without looking at the clock, and invariably check the counting after passage at the last wire. As to the manner of estimating fractions of seconds, my own custom, from the very first transit I ever observed (one, I believe, of γ Aquilæ), was to note the position of the star in the field at the clock-beat before reaching the wire, and again at the beat next after passing the wire, and estimate by eye the fractional part of the second corresponding to the position of the wire, and never in any case by mental estimation of the fraction of time elapsed between the preceding clock-beat and the bisection of the star by the wire. Mr. Stone (whose loss all astronomers will so deeply deplore, and whose first coming to Greenwich I so well remember, as well as my own pleasant intercourse with him during his whole stay there), at the April Meeting of the Royal Astronomical Society (see 'Observatory' for May 1897, p. 190), mentions that one of the Radcliffe observers adopts a combination of the two methods with some success. Would this be with quick-moving stars? Of course, with slow-moving stars some modification naturally comes in. Thus in observing a close polar star such as λ Ursæ Minoris no question of distance traversed in a second arises; it is here simply a time estimation. With such stars it was my custom, on account of atmospheric tremor, to note when the apparent disk touched the wire, was bisected by the wire, and last touched the wire, and take for the observed time the mean of the three observations. I confess that I quite sympathize with Capt. Noble (see 'Observatory' as before, p. 189) on the difficulty of observing by eye and ear a bright star such as Capella.

The old Troughton transit and the accompanying mural circle remained in use at Greenwich until the end of the year 1850, the present transit-circle having been brought into operation at the beginning of 1851. The Troughton transit, which occupied the room used from 1851 to 1897 as the Astronomer Royal's official room, was a very pleasant instrument to work with. The transit-clock stood then encased in a stone pier, the pendulum being visible through a plate of glass fixed in front, and, if my memory does not deceive me, the letters O and E were painted in black on the stone, one on each side of the glass front. In observing southern stars it was almost impossible to get wrong with the clock-beat.

The determination of the error of collimation of the Troughton transit was a tedious operation, and could not conveniently be very frequently performed. This difficulty seems to have impressed Sir George Airy, who, in designing the new transit-circle, provided fixed collimators, giving greater facilities for determination of this element. For the new instrument it was arranged that the error of level should be found by reflexion of the wire in mercury. For the Troughton transit a level placed on the pivots had been used. The diameter of pivots of the Troughton transit was relatively small; that of the pivots of the new instrument is 6 inches. On account of the massive character of the new instrument there was, perhaps, less of irregularity in the changes of instrumental error; but a new thing appeared, a strongly-marked annual period in the errors both of level and azimuth, with, as regards level, a drop of the eastern pivot (relatively to the western one), which, proceeding with the time, went on for some years, accompanied by, superposed on the annual variation, a variation of level from day to day closely following variation of temperature. I then found that the errors of level and azimuth of the old Troughton transit were also subject to annual periods, different in character to those of the transit-circle and smaller in magnitude (see vol. xxix. of the 'Memoirs of the Royal Astronomical Society'). The transit-instrument of the Edinburgh Observatory had been previously found to be affected with periodical changes of level corresponding to changes in temperature of the rock below. Professor Turner, many years afterwards (R. Ast. Soc. Notices, vol. xlvii.) collected the results for level and azimuth of the Greenwich transit-circle for the 34 years 1851 to 1884 (I had only employed 8 years), and further discussed the difficult subject of the relation with temperature. To those interested in such questions it may be also mentioned that Mr. Finlay, Chief Assistant at the Cape Observatory, has since contributed to vol. ii. of the 'Annals of the Cape Observatory' a very interesting paper on the variations of the instrumental adjustments of the Cape transit-circle.

I have mentioned the Greenwich mural circle in use to the end of 1850. It may be interesting further to say that with this instrument stars of low altitude could be readily observed by reflexion; there are instances of observations at less than 15° altitude, but with the new transit-circle the fixed collimators much restricted the extent of sky over which such observations could be made. This inconvenience was remedied by the present Astronomer Royal by remounting the collimators so that they could be turned aside when their use was not required.

We have wandered away from clock-beats. May I now gossip a little about comparison of clocks by the method of coincidence of beats? one by which sidereal and solar timekeepers can be, one with the other, so accurately compared, or by which, by means of an intermediate mean solar chronometer, the difference between two sidereal clocks in an observatory can be so exactly determined. The observation seems to be a pretty easy one to make, since the gradual approach of the two sets of beats to coincidence, as the one gains upon the other, so prepares the ear to seize the true instant of coincidence, which becomes perhaps more accurately fixed by following the beats until the one set has advanced so as to begin to appear sensibly in front of the other. But yet I have known observers who were liable to be in such observations more or less uncertain. The accuracy of this method of comparison is seen when it is remembered that an error of even as much as 4 seconds in estimating the moment of coincidence represents, in a single comparison, only an error of $0^s{\cdot}01$ in the transfer of the time.

I once employed the method of coincidence of beats in a critical case. When it was proposed to use the submarine telegraph to determine the longitude of Cairo for the purposes of the 1874 Transit of Venus, we were not allowed to connect the end of the submarine cable at Porthcurno in Cornwall with the land line to Greenwich Observatory. Porthcurno had thus to be made an intermediate station, but still it was desired not to make it also an astronomical station—that is, it was wished to avoid the erection of a transit or other corresponding time instrument. In the longitude operations I had charge of this station, and using a mean solar chronometer for exchange of signals with the Greenwich sidereal clock, and a sidereal chronometer for exchange of signals with the Alexandria mean solar chronometer, it became necessary to compare frequently my solar and sidereal chronometers by coincidence of beats, an operation by which the Greenwich time was carried forward with great certainty. It has been by some, I believe, supposed that in this determination of longitude special precautions were not taken beyond what was necessary to secure a result sufficient for the transit of Venus work. This is a misapprehension. For other of the Transit of Venus stations independent astronomical methods for longitude had necessarily to be relied on. But there being, in the case of Egypt, the possibility of securing a telegraphic determination, the desire was from the first to use this favourable opportunity to make the determination as accurate as possible, quite independent of the needs of the Transit of Venus. In the first place, the submarine telegraph line between Porthcurno and Alexandria was, for ordinary speaking purposes, worked in several distinct sections; but when it was proposed to use it for longitude purposes, Sir George Airy declined to entertain the proposal unless, the different sections being joined up into one continuous cable, signals could be satisfactorily exchanged direct between Porthcurno and Alexandria through the whole length of cable, and preliminary experiments were made at Porthcurno some months beforehand to ascertain that this could be satisfactorily done. Further, Mr. Cromwell Varley kindly had his artificial cable set up at Greenwich to enable the observers to practice the observation of signals such as would actually be received on the submarine cable. The determination of the personal equation in transits of the Cairo observer was not quite satisfactory, but the longitude was on the whole by no means a bad piece of work. Signals were exchanged on four nights, three separate exchanges having on each night to be made, between Greenwich and Porthcurno, between Porthcurno and Alexandria, and between Alexandria and Cairo, in each section giving in one direction sidereal signals and in the other mean solar. The extreme difference between the concluded longitudes of the four nights was only $0^{s}{\cdot}12$.

There is, of course, or should be, progress in all things, and improvements are continually made. In our day determinations of longitude are executed with greater elaboration of apparatus and method than heretofore, but unfortunately it does not in all cases follow that this ensures a greatly increased accuracy. The telegraphic arrangements may be so perfect that the exchange of a very moderate number of telegraphic signals will determine the actual difference of the clocks of two stations with an accuracy practically equal to that to be derived from the exchange of hundreds of signals; but still there comes in, as before, the determination of local time and personal equation, with their uncertainties, which at times become baffling. Indeed we must all have experienced and been correspondingly disappointed in being compelled to acknowledge that, when the possible outstanding error in the determination of the value of an element is small, it will happen that, to obtain an improved result, a refinement of instrumental means and methods is required apparently out of all proportion to the slight increase of accuracy presumably gained. The fight for greater accuracy is often a continuous warfare. A new method of attack may turn out to be a forlorn hope; but if fortune at last smiles, then indeed does the patient enquirer become rewarded for all his, it may be, years of labour.

Speaking of telegraphic work brings to mind that it is now 43 years since, in the year 1854, the chronographic method of recording transit-observations (or, as it was then called, the American method, Professor Mitchell having previously successfully applied the principle at Cincinnati) was brought into use at Greenwich Observatory. Sir George Airy, in designing the large chronograph still in use at Greenwich, arranged that the record of seconds by the clock and of transits by the observer should be made on the moving paper by two separate pricking-points. I well remember my first night of work with the new method of transit by "touch," in which for quick-moving stars my custom always was to endeavour to make the contact complete simultaneously with the apparent bisection of the star by the wire—that is to say, I did not wait until the star was on the wire before moving the finger. It is not easy or possible to use the chronographic method for close polar stars, because of atmospheric tremor, through which the "touch" may be made too soon, as watching the star for a few seconds longer might reveal. Not only did an observer, by use of the chronographic method, become relieved from the necessity of so much clock-counting, but the duration of a chronographic transit being also much shorter (one third of the length of an eye-and-ear transit, not reckoning the time lost in taking up the clock second in the latter method), it became possible to secure, in a given time, a greater number of observations than by the old method, with less fatigue to the observer. The winter wind might bluster, the observer now remained impassive, unless, indeed, he was anxious to obtain reflexion-observations, when, should a malevolent puff of wind come at a critical moment to ruffle the placid surface of the mercury, such an accident would probably not be calculated to promote his continued calm, could we be there to see—or hear.

The chronographic system, rendering unnecessary the tedious comparison between different of the observatory clocks, was another advantage, since the time-observations at all instruments communicating with the chronograph became referred to the one sidereal clock, and the introduction of this system into observatories certainly marked a great advance. But what developments are we yet to see? There is, for instance, the new telegraphy without wires. Shall we become able to pass longitude-signals across the ocean without wires? Or will some day the time-observations made at all the principal observatories of the world become registered on one big central chronograph to be maintained in continuous action, each observatory having its own sympathetic pricking-point, from which the several observed times will be afterwards flashed back to the different observatories as required? Some staid spirits may weakly murmur of difficulty: let them hide their diminished heads, for this word is evidently one that in the coming time (in the new century, perhaps) should be erased from all dictionaries. W. Ellis.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Forster and the Perseids.

Gentlemen,—

Now that August is once more approaching, matters relating to the Perseid meteors become again of special interest. It is somewhat remarkable that, although their appearance has for a long time past been annual, the history of these cannot be traced nearly so far back as that of the Leonids in November. Quetelet, in his 'Catalogue des principales Apparitions d'Etoiles filantes' (Brussels, 1839), quotes as the earliest known reference to them Musschenbroeck's 'Introduction à la Philosophie naturelle,' which appeared in 1762, and contains the passage (vol. ii. p. 1061): "Stellæ (*cadentes*) potissimum mense augusto post progressum æstum trajici observantur, saltem ita in Belgio, Leydæ, et Ultrajecti." Up to that time, then, it would seem that all that had been noticed was the frequent recurrence of meteoric appearances in the month of August. The earliest published work, I believe, in which the date of the Perseid shower is given more exactly is the 'Perennial Calendar' of Thomas Ignatius Maria Forster, which appeared in London in 1824, shortly followed (1827) by the 'Pocket Encyclopædia of Natural Phenonema,' which includes (Part Fourth) a 'Rustic Calendar,' giving for each day either a prognostic or an indication relative to the flowering of plants or the migration of birds. The 'Encyclopædia' is compiled principally from the manuscripts and journals of the author's father, Thomas Furly Forster. Under date August 10 it states: "Falling Stars and Meteors most abound about this time of year." T. I. M. Forster told Quetelet that there was an earlier reference to this in a manuscript preserved in the library of Corpus Christi College, Cambridge, entitled 'Ephemerides rerum naturalium,' and probably the work of a monk about the end of last century. He published at Bruges, in 1843, an 'Essai sur l'influence des Comètes sur les phénomènes de la Terre, Seconde Edition, augmentée d'un autre, sur les étoiles filantes,' in one of the "Notes supplementaires" of which, after arguing against their planetary nature, an interesting passage occurs respecting their appearance, which may be quoted:—"Le nombre varie chaque année, elles sont en général très fréquentes la nuit du 9 encore plus nombreux dans celle du 10 d'Août. Mais quelques nuits avant et après ces époques un certain nombre est toujours visible, ce qui prouve que la cause de ces étoiles est progressive. Je me rappelle l'année 1811, le 10 d'Août, retournant de Walthamstow à Clapton, avec mon père, entre 8 et demie et 10 heures, je voyais un si grand nombre de ces météores qu'il était difficile de les compter, ils prenaient des directions diverses et laissaient après eux de longues trainées blanchâtres, visibles quelques secondes dans l'atmosphère, après que l'étoile même fut éteinte. Quelques-unes étaient très grandes, mais toutes distinguées par ces longues trainées blanches." The author concludes with the remark that these white trains proved the phenomena to be atmospheric and not planetary, as apparently it did not occur to him that the trains might be produced in the atmosphere, but not the meteors themselves.

Quetelet, in the memoir cited at the beginning of this letter, says that Forster informed him that there was a tradition in his native country (Ireland) that the shooting-stars which appeared in such abundance in August were the burning tears of St. Lawrence, whose feast is kept on the 10th of that month. But he was certainly mistaken in supposing that Forster was an Irishman; nor, indeed, does it appear that he ever was in Ireland, so that he probably first heard the expression somewhere on the continent, in different parts of which he resided during the latter part of his life. He was a native of London, and, according to his own account in 'Recueil de ma vie,' was born in Threadneedle Street on the 9th of November, 1789. His paternal ancestors were long located in the northern part of Northumberland, but lost their estates through taking part in the Civil War of 1715 on the side of the Stuarts; his grandfather, Edward Forster, settled at Walthamstow, Essex, in 1764, soon after the birth of his eldest son, Thomas Furly Forster, in 1761. Thomas Ignatius Forster was his eldest son, and died at Brussels in 1860. He was an early Fellow of the Astronomical Society, and took part in founding a short-lived Meteorological Society. On the 3rd of July (the date on which I am writing this) he discovered a comet, which was observed the same night at the Royal Observatory, Greenwich. It was brilliant for a time, and continued visible for about four months; but Forster was anticipated in the discovery by Tralles, who noticed it on the 1st of the same month, and it was also seen at Berlin on the 2nd. Yours faithfully,

Blackheath, 1897, July 3. W. T. LYNN.

The Metric System.

GENTLEMEN,—

In the number of the 'Observatory' for June 1896, page 251, it is suggested, in an extract from the 'Times,' that in certain cases the French decimal system of money is less advantageous in practice than is the English system. As an example, it is said that "it is not difficult to see that it is pretty easy to work out mentally the value of, say, $1\frac{3}{4}$ yards of ribbon at $3\frac{1}{2}d.$ per yard, while it is very difficult to work out mentally 1 metre and 75 centimetres at 35 centimes a metre." In what way is this difficult? In the French system we can multiply 35 by 2 and subtract an eighth of the product, which gives 61 centimes and a fraction. In the English system $3\frac{1}{2}d. \times 2 - \frac{1}{8}$th of the product gives 6*d.* and a fraction of a farthing. A comment on the above is that "one and three quarters is treated like one and a half; but 175 passes into the domain of three figures." But does anything prevent 175 being similarly taken as 150? though I do not think I see the point of the comment. As to which, French or English, is the better plan, who is to judge? Those used to English coinage may prefer English reckoning, whilst those used to French may prefer the French reckoning. The person who can, without bias, say which system is abstractedly the more convenient may not exist, as we are all in such matters creatures of habit, and usually prefer that to which we are most accustomed, though it may not be the best. Consider another question of international difference. The centigrade thermometer scale is more scientific and natural than that of Fahrenheit, but how many of us can realize what amount of heat 25° of the centigrade scale represents? Say that the temperature is 77°, we are then quite at home. Here we prefer in practice that to which we are accustomed, although we know that it is not the best, or, indeed, the most convenient, since the Fahrenheit scale does not directly indicate degrees of frost.

Yours faithfully,

1897, July 20. W. ELLIS.

The Lunar Eclipse of August 27, B.C. 413.

GENTLEMEN,—

Canon Taylor, referring to my letter in the May number of the 'Observatory,' calls my attention to a remarkable statement in the late Prof. Freeman's 'History of Sicily,' vol. iii., which will amuse those of your readers who have not seen it. Speaking of the eclipse of the Moon which occurred on the 27th of August, B.C. 413, and so frightened Nicias and the Athenian army in Sicily that it caused the delay in the retreat from Syracuse which led to the destruction of the whole army, the Professor says:—"The unscientific mind finds it far easier to understand how the Moon can cast her shadow on the Sun than how the Moon herself can be entangled in the shadow of the Earth." Surely it is quite as difficult for the scientific as the unscientific mind to comprehend how the Moon or any other body can cast its shadow on the Sun from which it derives its light. Canon Taylor reviewed this volume in the 'Academy' for May 14, 1892, and justly characterizes the misconception as astounding, whilst pointing out that it cannot be a mere slip or misprint, because it is repeated in the Appendix. The fact is that Freeman utterly ignored all modern knowledge and threw himself astronomically into the position of the ancients of those times, though even so his language is a little loose. They do not seem to have grasped the idea, simple as it is to us, of opaque bodies like the Earth and Moon casting a conical shadow on the side opposite to that of the Sun; therefore they found it more easy to understand a solar eclipse than a lunar, because it seemed natural that the Moon should sometimes pass wholly or partly between us and the Sun, obscuring its light partially or totally (as Pericles illustrated the matter to his boatman by the interposition of his cloak), but were at a loss to conceive what body could come between us and the Moon, cutting off the whole or a part of her light.

The moral of the mistake in question is this, that the elementary facts of astronomy should be more generally inculcated; and there is no reason why these should not be taught to young people before they can enter on a mathematical study of the science. Yet, when I recently proposed at a meeting of Convocation of the University of London that this should form a part of the entrance examination, I was opposed (by a doctor—*not*, I need hardly remark, a doctor of science) on the ground that elementary astronomy does not help to a training in habits of observation. The confusion of thought here is between technical and professional observing with a telescope and ordinary observation with the eyes, which should precede the other, and of which young people are quite capable.

Yours faithfully,

Blackheath, 1897, May 15. W. T. Lynn.

OBSERVATORIES.

Cambridge University.—The opening paragraph of the Report which the Observatory Syndicate made to the Senate on 1897 June 1 is of such importance that it deserves quoting *verbatim*. The Syndicate report "that the term of five years for which Mr. Newall undertook to take charge without salary of the Newall Telescope has expired. They are glad to say that they have received from Mr. Newall an offer to forego the receipt of a salary for a further term of five years. They think, however, that the provision of a stipend for the Newall observer is an urgent claim on the funds of the University."

Beyond this, the Report consists of two statements by Sir Robert Ball and Mr. Newall respectively on the work of the two branches of the Observatory during the period 1896 May to 1897 May. The first of these announces that the zone +25° to +30° of the 'Catalog der astronomischen Gesellschaft,' which has been the staple work of this Observatory for the last 25 years, is now published. We offer congratulations to Mr. Graham on the completion of his work, and hope to take the opportunity of reviewing this on some future occasion. Sir Robert Ball also reports that the preparation for press of the complete Catalogue, which is to contain the individual observations of each star, is well advanced and will be printed by the University Press shortly. Discussion of these observations has been made, showing as a result that the probable error of an observation in R.A. is $0^{s}{\cdot}06608 \pm 0^{s}{\cdot}0004$, in Decl. $0''{\cdot}5079 \pm 0''{\cdot}0031$.

The making of the new Photographic Telescope, now in the workshop of Sir Howard Grubb, is being pushed on, and plans for the building have been prepared. It is possible that this may be erected during the present summer. It is proposed to devote this new instrument to the determination of stellar parallax, and a scheme of operations is being devised.

The subject matter of Mr. Newall's report was given by him in his paper to the Royal Astronomical Society which we reported in our last number. As supplementing that short abstract it may be stated that the early trouble Mr. Newall spoke of was due to the fact that he considers it essential in determination of velocity of stars in the line of sight by comparison of spectra, that the comparison spectrum should be fixed irrevocably on the plate by the instrument, and that its position should not be at the control of the observer; he tried to make arrangements to effect this, but in spite of the greatest care, comparison with the Potsdam results showed that a displacement such as it was designed to obviate did exist, and this was traced after much labour to the unpremeditated action of a spring on the frame of the spectroscope. This difficulty has been got over, and the spectroscope is now in satisfactory working order, as will be judged from the following comparison of the measures made by Mr. Newall with those made by Profs. Vogel and Scheiner at Potsdam. The results are given in kilometres per second:—

	Newall.	Vogel.	Scheiner.
α Tauri	+49·2	+47·6	+49·4
α Orionis	+10·6	+15·6	+18 8
α Canis Min.	− 3·5 − 4·9	− 7·2	−10·5
β Geminorum	− 0·7	+ 1·9	+ 0·4
γ Leonis	−42·3 −37·5	−36·5	−40·5
α Boötis	− 5·9 − 6·8	− 7·0	− 8·3

Belopolsky's result for α Boötis is $-5{\cdot}7$, and Keeler's $-6{\cdot}8$. In the column under the name "Newall" the results of two distinct determinations are given in some cases.

The earlier photographs, although of little value for the purpose for which they were intended, viz., for determination of velocity of approach or recession of stars, nevertheless form valuable material for the study of the nature of the spectra of various stars.

Royal Observatory, Edinburgh.—The seventh annual Report of the Astronomer Royal for Scotland, being the first issued from the new Observatory on Blackford Hill, is largely devoted to the history of early arrangements in setting up instruments and difficulties due to the flitting to the new home. The Transit Circle is at present used for time observations, and it is satisfactory to find that the settling of the piers which were put up in May 1893 has produced errors of only 6″ and 2″ in level and azimuth respectively.

The Time Service and meteorological observations have been carried on without intermission since the beginning of 1896. Since August 1896 the bifilar pendulum apparatus for automatically recording earth-tremors has been in action, and on several occasions deflections corresponding to earthquake shocks have been recorded. As to astronomical observations, Dr. Halm has used the 15-inch refractor for observations of positions of comets and minor planets, which have been communicated to the 'Monthly Notices,' and also with the large Cooke spectroscope for observation of planets and nebulæ.

The new reduction of the right ascensions of stars observed by the late Prof. Henderson at Calton Hill in the years 1834–45 has been continued. It seems that some discordances have been noted in these observations, which have been ascribed to two causes. First, it was suggested by Prof. Henderson, and afterwards by Piazzi Smyth, that the level and azimuth errors were dependent on the temperature, as registered by a thermometer placed a few inches beneath the soil. Corrections due to this cause have been evaluated and applied. Secondly, there seems to have been a large difference in the recorded transits, according to which end of the axis of the instrument, which was used in both positions, was east or west. It has been assumed that this difference is due to want of symmetry in the illumination of the field of view by the lamp. By applying an "Illumination Correction" as it is termed, the accidental errors have been greatly reduced, and Dr. Copeland thinks that very good results are to be expected from these observations of Henderson.

Paris.—Very naturally M. Loewy opens his Report to the Observatory Council for the year 1896 with a graceful tribute to the memory of his lamented predecessor, M. Tisserand. He next proceeds to refer to two other events of the year, the Astrographic

Conference, and the Conference on Astronomical Constants, which took place at Paris in May last. The work of the Observatory is conveniently reported on in sections, an arrangement which we will here follow.

Meridian Observations.—With the great meridian circle observations have been made for the revision of the declinations of the fundamental catalogue of the Paris Observatory, a work which was begun in May 1895. The Gambey instruments and the transit-circle "of the garden" have been devoted to observations of stars of Lalande's catalogue, the revision of which is proceeding apace. Altogether 12,954 meridian observations of stars have been made and 237 of the Sun, Moon and planets.

Equatorials.—The great *équatorial coudé* has been used on several nights to make photographs of the Moon, which will form part of 'l'Atlas photographique de la Lune,' the first volume of which has been published during the year. M. Loewy relates with regret that the weather was very unfavourable during 1896 for this, as well as for the other classes of work. MM. Loewy and Puiseux have exercised themselves to find a means of making the photographic plate automatically follow the motion of the Moon in both elements, and have contrived an arrangement for making the plate move with respect to the telescope. They are of opinion that the shortness of exposure required for the Moon renders steering by hand impracticable. The smaller equatorial has been used for observations of small planets, and the equatorial in the western tower besides being used for the ordinary observations of comets, occultations, and phenomena of Jupiter's satellites as they occur, has also been used by M. Bigourdan to make observations for a catalogue, on which he is engaged, of the places of all nebulæ visible from Paris.

The Astrographic Chart.—140 Catalogue plates have been taken during the year, but the weather has only permitted the making of three chart plates of the ecliptic. Each of these is exposed three times for an hour. Sixty-one catalogue plates have been measured, which give an average of 496 stars for each plate, and the reduction of these is proceeding.

M. Deslandres's section on spectroscopy relates mainly to the eclipse expedition to Japan. Before April, however, the researches which are the usual work of this department were continued. Nineteen photographs of spectra of stars for determination of velocity in the line of sight were taken and observations of the chromosphere and prominences were made.

The Report refers to the Paris Catalogue of stars, of which the third volume (12^h to 18^h) has been published during the year, as well as the corresponding volume of observed places. It is expected that this will be finished in the year 1900.

BIDSTON OBSERVATORY, LIVERPOOL.—From his Report for the year 1896 we gather that Mr. W. E. Plummer, the Director of this Observatory, devoted his energies during the year mostly to meteorology. A Dines Tube Anemometer has been set up in a suitable position at a height ten feet above the older anemometers, and a comparison of the results given by these instruments, and an interesting description of the determination of the scale of the Dines anemometer, fills a large part of the Report. Of the astronomical instruments, the Transit has been used for the determination of Time, and has also been used by students, under Mr. Plummer's superintendence, for educational purposes. Observations of comets have been made with the equatorial, which continues to perform satisfactorily, and observations of Encke Swift, Perrine, and Brooks' Comets have been communicated to the Royal Astronomical Society. But the weather during the year has not been very favourable for astronomical work.

NOTES.

COMET NOTES.—Ast. Journ. No. 406 contains a valuable series of observations of Brooks' Periodic Comet (1889 V. and 1896 VI.) made at the Washington Observatory, and extending up to Feb. 4 in the present year. Special search was made for some of the companion comets which were seen in 1889, and two suspicious objects were in fact observed, but they showed no motion and were probably nebulæ.

We gave last March in these columns a search ephemeris for d'Arrest's comet, computed by M. Leveau. The comet was in fact picked up on June 28 by Mr. Perrine at the Lick Observatory, the R.A. being some 4 minutes less than that of the ephemeris, the declination practically the same. Perihelion passage occurred about May 21 and the brightness is now steadily declining.

The following rough ephemeris (for Paris midnight) is from that of M. Leveau, corrected by the observation of June 28:—

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Aug. 1....	3	27	0	6	7	Aug. 9....	3	43	0	5	32
5....	3	35	14	5	51	13....	3	50	18	5	10

It will be seen that the comet is a morning object, rising some 4 hours before the Sun on Aug. 5. A. C. D. C.

MINOR PLANET NOTES.—Planet 350, discovered by Charlois 1892 Dec. 14, has been named Ornamenta (Ast. Nach. 3429).

A. C. D. C.

THE CONSTANT OF PRECESSION.—It will be remembered that at the Conference held at Paris last May it was resolved by the Superintendents of the four National Ephemerides that from the year 1900 the same fundamental constants of astronomy should be used in the computations of all these publications, and the task of determining the exact value of the constant of precession to be used was left in the hands of Prof. Newcomb. No time has been lost in doing this, and, without waiting for the official publication, Prof. Newcomb has published an abstract of his research and his result in 'Astronomical Journal,' No. 405.

It is scarcely necessary to say that for the determination of this constant it is necessary to have as data the observed places of certain stars at two epochs separated by a considerable interval, and so far the determination is similar to that of proper motion. In fact the problem is one of which the rigorous solution is indeterminate, since the data supply one equation with two unknowns, precession and proper motion, the relative values of which are adopted by discussion of various extraneous considerations.

Prof. Newcomb took as the basis of his research the stars of the Auwers-Bradley catalogue. The apparent proper motions of all these have been computed by Dr. Auwers, so the data were ready to hand. Stripped of all detail, and on the assumption that real proper motions are distributed among the stars so that the algebraic sum over the whole sky is zero, the mean of these apparent proper motions would give a correction to the adopted constant of precession. But the assumption certainly does not represent the facts, the distribution of actual proper motion does not follow the law of error, and the motion of the solar system in space causes an apparent proper motion of different value in different parts of the sky. These things Prof. Newcomb naturally has taken into consideration, and determined the constant in various ways.

First by what he calls the statistical method. Here, dividing the stars into quadrants of right ascension, and the stars of each quadrant into groups according to magnitude, he finds a proper motion in each group, so that the number greater and the number less than this are equal, and combines these mean values for the groups to find the precession constant. In his second method, making the effect of parallactic motion, or proper motion due to the solar motion, an unknown, as well as the precession constant, he forms equations for individual stars, and solves these to find values of the constant and of the solar motion. Thirdly, he uses a method of dividing the stars into zones and regions, and then another method by choosing coordinates so that the parallactic motion is eliminated. The research also contains a determination of the value of the constant from discussion of Dreyer's comparison of Lalande's catalogue with Schjellerup, and of Boss's comparison of Lalande with the Albany catalogue. The results obtained by these several methods differ somewhat among themselves, but Prof. Newcomb has, after due consideration, adopted a mean value.

The resulting values, as compared with those of Struve-Peters for the year 1850, are given in the following table:—

	Struve-Peters.	Newcomb.
General Precession	5025″·24	5024″·53
Luni-Solar Precession....	5038 ·23	5036 ·84
Value of 100 *m*	4607 ·65	4607 ·11
Value of 100 *n*..........	2005 ·64	2005 ·11

THE GEGENSCHEIN OR ZODIACAL COUNTERGLOW.—This phenomenon, which possibly belongs to the domain of meteorology rather than to astronomy, is little known to most persons, but as it is observed by the naked eye it might be a subject of interesting study to many. Prof. Barnard, of Yerkes, has made this object peculiarly his own, and published in 'Ast. Journal,' No. 403, a little table of results from observations made during the past fourteen years. In an earlier number of the same journal (No. 168, 1888 March 23) he wrote: "When first seen in the fall it is a roundish mass of hazy light about ten degrees in diameter; and though probably very slightly so, it is not perceptibly brighter in the middle. Observations on successive nights show that it has a decided motion eastward among the stars, and that it lies on, or very close to, the Earth's orbit, and its longitude differs by very nearly, if not exactly, 180° from that of the Sun. After passing the vernal equinox in the latter part of September, a perceptible change takes place in its form, a faint zodiacal band becomes visible, extending from it some distance on each side, and the counterglow assumes an elliptical form; later it becomes more flattened and the band of light more distinct, until, finally, this band is seen at midnight connecting the evening and morning zodiacal lights, and the counterglow a mere swelling, scarcely denser and but little broader than the band." The recently published paper corroborates the above statement to some extent. The appearance in March and April appears to be similar to that in September and October, but this is not so well established, and, as has been before noticed, the Gegenschein lags behind the Sun, that is, its longitude from the Sun has been observed to vary from 179°·4 to 179°·9. Its average latitude is about +0°·7. The phenomenon is not equally well seen in all places and under all atmospheric conditions, but Prof. Barnard says that it does not require a very transparent sky to see it. At Yerkes it is so easily visible that nearly everyone connected with the Observatory has seen it, though they were not familiar with it before.

THE ROTATION OF JUPITER'S SATELLITES.—In our last number we briefly mentioned a result published by Mr. Douglass, of the Lowell Observatory, as to the time of rotation on their axes of the third and fourth satellites of Jupiter. In Ast. Nach. No. 3432 he gives the observations on which the result for the third satellite depends. The observations consist of drawings of markings seen on the satellite on each day from Feb. 20 to March 12, except Feb. 26 and March 11. The markings are shown by three or four lines drawn with the utmost distinctness, their arrangement showing a different conformation on consecutive days in all cases, but the same, or nearly the same, conformations recur every seven days. Taking the markings for seven consecutive days, Mr. Douglass joins these together to form a map of the satellite. Now, says he, if this be a true map, the conformations can only be seen at the times they have been seen if either the satellite rotates directly in about 21 hours, in a retrograde direction in 27 hours, or in a period of about 7^{d} direct motion. The first two hypotheses are excluded because, from drawings made at epochs separated by intervals of 3 hours, the markings are essentially the same, showing that the rotation during 3^{h} does not approach one-eighth of a revolution. The exact times of the drawings of the longer series are not given, so that there is no means of tracing the exact result; but Mr. Douglass gives this as $7^{\mathrm{d}}\ 5^{\mathrm{h}}\cdot 1 \pm 1^{\mathrm{h}}\cdot 2$, which is 1^{h} longer than its period about Jupiter.

MICROMETRICAL MEASURES OF THE JOVIAN SYSTEM.—Herr Brenner has favoured us with the following early results:—

"The reduction of my measures of Jupiter and his satellites give the following mean results:—

Jupiter, polar diameter	36″·135	±0″·0617
,, equatorial diameter....	38·539	±0·0427
Satellite I., polar diameter	1·060	±0·0097
,, II., polar diameter....	0·958	±0·0138
,, II., equatorial diameter.	1·063	±0·0070
,, III., polar diameter....	1·504	±0·0401
,, III., equatorial diameter	1·704	±0·0217
,, IV., polar diameter ...	1·345	±0·0010
,, IV., equatorial diameter	1·550	±0·0161

"A detailed report on these and other measures will soon be published in the 'Astronom. Nachrichten.'

"Manora Observatory." "LEO BRENNER."

THE ASTRONOMICAL AND PHYSICAL SOCIETY OF TORONTO.—We have had sent us the volume of 'Transactions of the Astronomical and Physical Society of Toronto' for 1896, a publication the reading of which always supplies something of interest. It always seems to us that this Society is in many ways a model popular scientific society—not too scientific, but just scientific enough. There is not very much astronomy in the book that one could quote, but mention must be made of the President's (Mr. John A. Paterson) excellent address on the progress of astronomy during the year. He, by the way, calls the notice issued by the Royal Astronomical Society relative to the admission of ladies to its meetings a "haughty rule of exclusiveness," and congratulates Toronto that she is not as London is. Mr. Lindsay gives a chapter, as sequel to one in 1895, on the history of the 'Nautical Almanac,' in which he wanders away into descriptions of other almanacs and diaries, and gives much information in a pleasantly discursive way. Mr. Stupart, Superintendent of the Meteorological Service of Canada, attended the Meteorological Conference at Paris in September 1896, and relates his experiences there, and also at some English observatories which he visited; but he seems to have taken home with him an idea which we should like to correct, namely, that at Greenwich, in the measurement of sun-spots, "less importance is attached to the position of sun-spots than to the total area covered by them." We are really unable to attach any accurate meaning to his remark. Positions and areas are both measured and reduced, it is believed with equal accuracy—that is, if it be possible to compare the accuracy of the measure of an area with that of a distance. Certainly there is no idea in any one's mind that the positions are unimportant. We congratulate this Society on its evidently healthy condition.

THE opening celebration of the Yerkes Observatory is fixed for September 30. No doubt astronomers who go to Toronto for the meeting of the British Association will find it convenient to stay for the ceremony at Yerkes. We hear that a programme of conferences on astronomical and astrophysical subjects has been planned, which may occupy about a week. The unfortunate accident to the floor of the great dome is being remedied as quickly as possible, and the telescope may be in use by the early part of August.

THE U.S. NAVAL OBSERVATORY.—From various reasons lately we have suspected that things were not going smoothly at the United States Government Observatory. A note in the July number of 'Popular Astronomy' tells us this openly, but leaves us as much in the dark as ever as to the exact form the trouble is taking, and as to the reason for it. The note says:—"It cannot be due to the change in administration, for several of the men are about ready to be retired on account of age and long honourable service, nor for differences of scholarly opinion, because the routine work of that great Observatory generally involves only matters in astronomy the debateable part of which science has settled long years ago . . . . . . What is the matter with the working staff of our National Observatory?" We wonder whether the writer of the note is as ignorant as he would have us believe, or is this "wrote sarcastic"?

THE RADCLIFFE OBSERVATORY.—The post of Radcliffe Observer in the University of Oxford, vacant by the death of Mr. E. J. Stone, has been filled by the election of Dr. A. A. Rambaut, Director of the Dunsink Observatory and Astronomer Royal for Ireland, to whom we offer our warmest congratulations.

WE also have to congratulate Mr. G. J. Symons, F.R.S., the Editor of our contemporary the 'Monthly Meteorological Magazine,' who has been awarded the Albert Medal, with the approval of the Prince of Wales, for services rendered to engineering science and the country generally by recording and tabulating rainfall.

We are informed that Mr. Joseph Lunt, B.Sc., has been appointed Physical Assistant at the Royal Observatory, Cape of Good Hope. He will probably be placed in charge of the McClean telescope.

The occultation of the Pleiades on the morning of July 24 (civil day) was well observed at Greenwich.

From an Oxford Note-book.

At the June meeting of the Royal Astronomical Society the Chairman read the Address which it was proposed to present to the Queen in this year of Great Jubilee. The address was presented to the Prince of Wales, on behalf of the Queen, on July 21, by a deputation consisting of the President and one of the Secretaries. The President was the fortunate possessor of a beautiful Court dress, and looked truly magnificent; and even the Secretary had bought a new coat for the occasion, though he took advantage of the gracious permission accorded to those "not accustomed to Court dress" to appear in ordinary garb. The deputation was number 16 on the list of some 30 or more, and came at the tail end of the first draft of 16 admitted into the ceremonial chamber. The critical moment was thus anticipated from a gradually decreasing distance, during which the deputation observed the Court gestures of its predecessors and nervously endeavoured to select the most appropriate from the variety submitted for inspection. In the event the President could scarcely bear to part with the Address after all; and only yielded it up on the special request of His Royal Highness: and the deprivation caused him so much emotion that he turned hastily away without the gracious reply, for which, however, he was persuaded to return. But although the deputation thus secured to itself a double or treble portion of the Royal attention, the whole proceedings only occupied a few minutes. They commenced at 11.30 sharp, and by 11.45 deputation number 16 was getting its hats and umbrellas.

On the occasion of the former or Lesser Jubilee in 1887 the Queen received a similar address in person from the President and Secretary of the time: and Mr. Knobel took the roll-book and obtained Her Majesty's signature as the Patron of the Society, a piece of thoughtfulness for which the Society is much indebted to him. The book was not signed at the time of presentation of the Address, but was taken to the Queen by the Lord Chamberlain after the ceremony was concluded; and it is whispered that it was examined with considerable interest by the Royalties assembled *en famille.*

In a notice in the 'Daily Telegraph' of the splendid Naval Review at Spithead on June 26 Mr. Edward Dicey writes:—"An old salt on board remarked, in my hearing, that the guns (I omit the expletives) would bring down the rain as sure as eggs is eggs. The weather, however, looked as if summer had set in to last for months, and the prophecy met with the derision which prophets have always undergone since the days of Cassandra." Nevertheless we had a violent storm within an hour or two afterwards. I do not know how the evidence for guns bringing down rain stands at present; but I imagine there is some on the positive side: and there are occasions when it would seem well to give ourselves the benefit of the doubt. Thus if rain is badly wanted, and guns are handy, their efficacy might be tried in favour of the farmer; though of course it costs money to fire guns, and the remedy may be more expensive than the disease. There is, however, another side to the question which affects the astronomer. Dr. Common declares that they might have had fine weather in Norway for the eclipse of last August but for the heavy firing with which a Royal birthday was celebrated. Now an eclipse is an occasion when it is important not to throw away even the slightest chance of fine weather; and thus if there is the merest jot or tittle of evidence for firing imperilling the chances, it seems advisable to abstain from it. Here there is no certain expenditure for uncertain gain, but, on the other hand, a certain saving, and the regulations of our Services could be relaxed in such exceptional cases without hurting anyone.

THE subject of personal equation seems to have a great fascination for those whom I will venture to call the astronomical public. Prof. T. H. Safford recently contributed a paper to the Royal Astronomical Society on personal equation which, to speak frankly, did not seem to me to contain anything very new or important, but which the more popular magazines have nearly all noticed at some length. It will be almost a pity if we manage to get rid of personal equation by the photographic method, as some of us hope; for it will remove a link between the professional astronomer and the *dilettante* (this is a politer term than "astronomical public"; I wish I had thought of it before) in the shape of a topic at once important and intelligible to both. Perhaps we may adopt towards these attempts to eliminate the observer the attitude of the old golfer, to whom the merits of a new patent invention calculated to immensely improve driving had just been expounded, and who sighed, "Well! all I can say is, I hope it won't succeed."

HAVING spoken rather disrespectfully of Prof. Safford' paper, it seems only fair to say something on the subject myself, so that he can have a shot at me, if desirable. Some years ago I made some experiments which seemed to show that personal equation might be got rid of if by any means the observer could make his observation with one sense only. In the eye-and-ear method he uses two senses, as the name implies; and in the chronographic method he uses sight and touch. The want of coördination between the pair of senses introduces personality. But if we make the clock-beats visible as well as audible, so that the eye alone is used, we ought to get rid of personality. A rough experiment was first made as follows:—On the wall of a room provided with windows and shutters, a bead representing an artificial star was made to transit a series of black lines representing wires, and seconds were tapped off with a hammer. Several observers noted the transits, and their results were affected with sensible and constant personalities. The hammer was now laid aside and the room darkened by closing all the shutters, except that one shutter was flashed open every second. The bead was made to travel over the wires as before, and its transit observed by noting its successive positions at the only instants when it was visible. The observers then all agreed—the eye alone was concerned, and their personalities disappeared. An attempt was then made to apply the method to the transit-circle: a screen in front of the eyepiece being lifted every second by an electromagnet. But it was found that the action of the electromagnet varied with the inclination of the telescope, and I had not time to follow the matter up and overcome the practical difficulties.

THERE seems, however, little doubt that the photographic method will give us information nearly, if not quite, complete on this subject. Mr. Hinks has shown (M. N., vol. lvii. April) how photographs taken in the ordinary way with an equatorial will detect variation of personality with stellar magnitude; and some form of photographic transit will doubtless be in regular use before long. Several have already been proposed or are actually at work; and Prof. Wadsworth, of the Yerkes Observatory, tells me that he has another form in view. His name reminds me to draw attention to the substitute for spider-lines in the shape of silvered quartz-fibres. The idea is, of course, not new, for it is some time since Mr. Boys invented quartz-fibres, and, to my own knowledge, at least one English instrument-maker has tried them for use in telescopes. But he did not succeed with them for some reason—perhaps he did not silver them; or there may have been some other cause of failure. Thus it does not seem superfluous to call special attention to Prof. Wadsworth's remarks on the subject (M. N., vol. lvii. June), and side by side with this paper is one equally interesting and important on sky-illumination, which I have not yet fully digested, and so say nothing further about.

CAN any kind reader tell me when and where Mr. Proctor said something about a comet falling into the Sun this year, and "bursting" things generally? Several nervous correspondents have written on the subject, and I could only tell them that I did not know the original reference at all; but that Mr. Proctor must certainly have modified his views before his death, for he had written (almost completely) a great book on 'Old and New Astronomy,' in which there is not only no mention of this impending disaster, but not even a reference to comets of any kind whatever!

THE OBSERVATORY,

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On the Photography of Planetary Surfaces, with an Introductory Note on the Application of Photography to Astronomical Research.

No one who has followed the wonderful development of astronomical photography within the last few years can have failed to be impressed with the belief that the time is not far distant when photographic records will take the place of direct visual observations in all but a very few, perhaps ultimately in all, of our fundamental astrophysical and astronomical investigations. It is necessary to refer to only a very few uses to which photography has of late been applied to bear out this view. The spectrograph, in the hands of such investigators as Huggins, Vogel, Pickering, Keeler, and Campbell, has already practically superseded the spectroscope in the study of stellar spectra. The spectroheliograph and photoheliograph, in the hands of Hale, Deslandres, Janssen, and others, have given us details on the solar surface that probably never would have been obtained by visual study; or if so, obtained only at an enormously greater sacrifice of time and energy. The same thing is true to even a greater extent as regards the study of the solar and terrestrial spectra. The results achieved by Rowland and by the investigators who have followed in his footsteps are the only arguments necessary to demonstrate the superiority in every way of photographic methods in such work. In the study of the infra-red spectrum the application of photographic methods of investigation "brings results superior both in quantity and quality to what formerly required many weeks or even months" (Presidential Address before the American Association for the Advancement of Science, 1895, Nature, Jan. 9, 1896, p. 236). The results which will be achieved by the systematic Astrographic survey of the heavens, now going on, will testify to the advantage of photography in star-charting and in the determination of relative stellar magnitudes. Think what a length of time would have been required to obtain the same data by visual observations, if, indeed, they could ever have been obtained.

The photographic work on nebulæ and star-clusters, initiated by Draper, Pickering, and Common, and followed up with such great success by Barnard, Wolf, Wilson, Russell, Roberts, and many others, has given us entirely new conceptions of the wonderful extent and complexity of the celestial world in the various stages of its development. Lunar photography, in the hands of the Henry Brothers, Loewy and Puiseux, and Holden, has given us for the first time authoritative and complete charts of the surface of our satellite. Initiated by the late Professor Pritchard in 1886, photography has already demonstrated its value in the accurate determination of stellar parallax. In the hands of Hagen, Marcuse, and others the photochronograph is proving its value in meridian and zenith telescope observations. More promising still are the plans for applying photography to meridian work, recently described by Russell and by Turner. The advantages of photography in the determination of longitudes have been recently described by Captain Hills (Meeting of R. A. S., May 1897; 'Observatory,' June 1897). And now, last of all, the domain of double-star work, the stronghold of the older school of observers, has been invaded under the leadership of Christie at Greenwich. Photography has also proved its value for discovery in astronomical work, as evidenced by the discoveries of the ultra-violet lines in solar and stellar spectra, the discovery of Barnard's comet of 1894, and the discovery of many new asteroids, nebulæ, and faint stars.

But it is unnecessary to enlarge further upon this subject, which has been far more ably treated than I could hope to do by Russell in his recent Presidential Address: "Progress of Astronomical Photography" (British Association for the Advancement of Science for 1894). His introductory remarks, which I take the liberty of quoting below, express, I believe, the opinion of most astronomers and astrophysicists of the present day. He says:—"I will confine my remarks to a branch of one of our subjects which, within the past twenty years, has done more than anything else to accelerate the progress of our knowledge and to extend our grasp of the grand truths of astronomy. I refer, of course, to the application of photography to the wants of astronomical research. Coming in at first as another possible aid to the observer, it has already shown us that in many cases the observer must stand aside while the sensitive photographic plate takes his place and works with a power of which he is not capable; and I feel sure that in a very few years the observer will be displaced altogether, while his duty will be done by a new sensitive being, not only taking in the visual ray, but also the actinic rays in the ultra-violet—a being not subject to fatigue, to indigestion, to east winds, to temper, to bias, but one above all these weaknesses, calm and unruffled, and living with all the world shut out, only to catch the fleeting rays of light and tell their story.

"It has been well said by a very gifted writer [Miss Clerke, 'History of Astronomy'], that 'the invention of the telescope itself does not mark an epoch more distinctly than the admission of the camera to the celestial armory. All the conditions of sidereal research in especial are being rapidly transformed by its co-operation.'

"By this new lever the progress of astronomy is being urged forward at a rate which accomplishes more in ten years than was possible in a hundred years by older methods. Its whole life-history covers but fifty-three years, and its infancy and youth were cramped by the want of means of existence and growth; but its latter years have been marked by a vigor which has done so much that I shall have scarcely enough space to more than mark the stepping-stones in that onward march; to trace the details would take volumes."

To show, however, that there are still some astronomers who are not in sympathy with these ideas, I quote from another paper which appeared some few months after Mr. Russell's address had been published (Pop. Astron., Feb. 1895). This writer says (the *italics* are mine):—"The majority of students are generally encouraged to take up some *easy* task, and accordingly of late years the *subject of photography* has been a great temptation to the student, the amateur, *and to a large number of astronomers.* It is hardly necessary to add that while amateur photography is a *harmless amusement, it is not likely to lead to important scientific results.*" Another remark of the same writer (Pop. Astron., April 1897), is so very apropos of these opinions that I cannot refrain from quoting it also in this connection:—"Posterity will be able to decide whether persons inclined to discredit observations on the simple ground of prejudice are in any way superior to those in past ages who for similar reasons have persisted in being behind and in ignorance of some of the most important advances of these times."

Considering broadly the question of the applications of photography to astronomical and astrophysical research, we see that there are two general advantages which photographic methods possess, which have been decisive in determining respectively its use in two distinct classes of work. 1st. Rapidity and range in obtaining records which may afterward be examined and discussed at leisure; this has determined its use in cases where the time of observation was limited, as in the case of eclipses, occultations, transits, &c., &c., or where it is desired to accomplish, with a minimum expenditure of time, labour, and instrumental equipment, a large amount of work of a routine character, such, for example, as star-charting; for determining either (*a*) position and magnitude, or (*b*) spectral character (many new nebulæ, asteroids, spectroscopic binaries, variable stars, &c., &c., and one new comet, have been discovered in this way). 2nd. Accuracy and sensitiveness in the delineation of detail, combined with freedom from "personal error" in the records obtained. These last advantages, which are not so generally recognized and acknowledged as the first one, are discussed more at length by the writer in another paper in the August number of 'Popular Astronomy' ("On a Comparison of the Photographic and of the Hand and Eye Methods of Delineating the Surface-markings of Celestial Objects"). It is these that render photography so valuable in its applications on the one hand to the determination of stellar parallax (Pritchard), to the determination of right ascensions (Turner, Russell, &c.), to longitude determinations (Hills), to the proposed double-star measurements (Christie); and on the other to the mapping of solar and stellar spectra, to the study of the solar surface and attendant phenomena (the solar prominences, faculæ, sun-spots, &c.), to the delineation of nebulæ and comets with a view of observing any changes that take place in these bodies, and to the charting of details on the lunar surface. And it is these advantages also that, as I pointed out in the paper just referred to, ought to make photographic methods pre-eminent in the study and delineation of the surface-markings of the planets. As yet, however, photography has not given us in the case of the planets results which are comparable in any respect with the advances made in other fields of astronomical research by its use. The best photographs of Jupiter, Saturn, and Mars so far obtained are those taken by Schaeberle, Holden, and Campbell at Lick, and by Pickering at Wilson's Peak; none of these show as much detail as is visible even with a comparatively small telescope at intervals of good "seeing" on these planets. Why is this? Conditions of definition being the same there is (up to a certain limit in size, which is determined by mechanical rather than by optical considerations) no reason why the photographic plate should not show in this case, as it has undoubtedly done in others, all and even more detail (on account of the greater resolving power, aperture for aperture, of the photographic objective) than can be visually observed. The principal reason for its failure to do this is, I believe, that given in my former paper ('Popular Astronomy,' August), viz., that when the time of exposure is of any length, as it must be in the case of the planets, the final photograph is "the resultant effect of many successive impressions which may differ a good deal at different times, owing to atmospheric disturbances." "The result is very much the same (though produced by somewhat different causes) as would be produced by printing from a negative or plate ..... kept in continual vibration during the printing process.......... The eye employs no such process of summation, but *makes the most of the intervals of good seeing*, being little influenced in forming the mental picture either by slight movements of the image as a whole or by occasional intervals of haziness and 'fuzziness' due to momentary atmospheric or instrumental disturbances." In photographing the solar and lunar surfaces and nebulæ and comets, this difficulty in the use of photography does not enter to the same extent for several reasons. In the first place, the exposure time for the Sun and Moon can be much shorter, consequently the image is much more likely to remain steady during the whole time of the exposure. In the second place, in the case of nebulæ and comets, the details to be photographed are not fine lines and sharply-defined objects like the lunar markings, but are of a cloud-like structure, more or less coarse and indefinite in character, and are therefore much less changed in appearance by atmospheric disturbances. Again, the apertures used in photographing these latter objects may, on account of this very coarseness in structure, be of comparatively small size, while retaining sufficient resolving power to show practically all the detail that exists. Finally, the *contrast* between such objects and the sky, upon which the question of showing faint details almost entirely depends, is, as I have shown in a recent paper ('Knowledge,' August 1897), a question of the linear aperture of the image-forming lens, and varies inversely as the square of the latter. Hence to show very faint details small apertures are absolutely essential, the very condition, fortunately, that reduces the atmospheric causes of difficulty to a minimum. It was also shown (ibid.) that within certain limits, principally determined by the range in methods of photographic development, the time of exposure is independent of the *angular* aperture of the image-forming lens, and dependent only on its absolute focal length, a conclusion quite at variance with the usual assumption, but one which follows at once from considerations of the *contrast* effect between image and sky.

But in photographing the planets there are no such conditions in our favour. In this case we are concerned not with the delineation of an object against the sky background, but principally with the delineation of detail of the face of the planet itself; therefore the time of exposure will vary according to the usual law, *i. e.*, for a given focal length inversely as the square of the angular aperture. Since we require a great focal length (actual or equivalent) in order to obtain an image of a certain size on the photographic plate (this should be at least 2 mm. to 3 mm. in diameter in order to avoid the necessity for too great subsequent enlargement or magnification, which might introduce false appearances by reason of a variation in the size and distribution of the silver grains), it follows that we must make use of an objective of considerable linear aperture in order to make the time of exposure reasonably short. And since this last condition (short exposure) is also one which is plainly essential to success for a number of reasons (of which perhaps the most important is the axial rotation of the planets and the consequent continual shift of the markings on their surfaces), it is evident that in this case it is not permissible to avoid difficulties due to atmospheric disturbances by reducing the linear aperture below a certain limit. Is there not, then, some other way in which the effects of these disturbances can be avoided in photographic work as they are (involuntary) avoided in visual work, by basing our impressions on what we see only during intervals of good seeing? Evidently they can be by an exactly similar method—*i. e.*, by exposing not continuously, but intermittently, and only at the instants of best definition, until the requisite total exposure time has been attained. The image must, of course, be "followed" very accurately between and during the successive steps of the exposure. Of this last very important condition more will be said hereafter.

This method is so simple and withal so effective in avoiding the effect of atmospheric disturbance that it seems surprising that it should not have been used or suggested before this. But, so far as the writer is aware, the universal practice heretofore has been to make the exposure continuous from beginning to end, except perhaps when it is interrupted merely for the purpose of correcting some accidental displacement. The idea of intermittent exposure at instants only of good definition first suggested itself to the writer last winter, and some experiments to test its advantages in the case of solar photography were immediately made with an enlarged image of the Sun. In the case of the Sun the exposure is, of course, continuous when once begun, but it seemed certain that in this case, as in the case of the planets, very much better results would be obtained if the exposures were made at instants of the best definition, than would be if the exposures were made at haphazard (as regards atmospheric conditions), as has generally been done heretofore. The instrument used in these experiments was Professor Hale's 12-inch Kenwood telescope, which had then just been set up at the Observatory but not yet adjusted. This instrument, designed originally for Professor Hale's spectroheliographic work on the Sun, was at that time provided with brackets at the upper and lower ends of the tube, designed to carry respectively a second 12-inch photographic objective (of the same focal length as the visual objective) and the enlarging-lens and camera-box ('Astronomy and Astro-Physics,' vol. iii. p. 610). Exposures were made by means of a single sliding wooden shutter driven by rubber bands and placed just in front of the photographic plate. Very promising results were obtained in spite of the generally bad atmospheric conditions which prevail here during the winter season. Both on account of the unfavourable weather and on account of the unfinished condition of the telescope (it not having been adjusted and the clock and slow motions not being in proper condition for accurate following), it was not possible to make experiments at that time in photographing the planets. A considerable amount of work was subsequently done on the Sun with a new form of rotating photoheliograph shutter (this will be described fully when some further experimental results have been obtained with it) designed with special reference to this method of exposing. This work, however, had barely passed the experimental stage when it became necessary to give it up temporarily, it having been decided that it was best to remodel the 12-inch and give it over entirely to visual observations. As soon as the 40-inch telescope has been completed the 12-inch photographic objective will be mounted on its tube, and these experiments will be resumed. But the many delays that have already occurred in the completion of this instrument, and the recent breaking down of the rising floor, make it seem likely that at least another season must elapse before an opportunity of putting the proposed method to test on the planets will present itself.

For this reason I have thought it best to describe (perhaps prematurely) a method of procedure which as yet lacks the stamp of actual trial, in the hope that some one else who has the proper instrumental equipment may be induced to give it a fair trial at once. And it may be well to describe also what has so far seemed to me (further experiments may lead me to change my opinion in regard to some minor points) the best instrumental equipment for photographing the planets. In this connection there are several points to be considered.

I. *Best Ratio of Aperture to Focal Length.*—It has been shown in a previous paper ("Conditions of Maximum Efficiency in the Use of the Spectrograph," 'Astrophysical Journal,' May 1896) that in order to obtain full photographic resolution we must have

$$\beta = \frac{a}{f} = m\frac{\lambda}{e} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where a and f are respectively the linear aperture and focal length of the photographic objective, λ is the mean wave-length of light, e the mean diameter of the silver grains, supposed to be practically in contact with each other, and m a constant which will vary from 0·25 to 0·35 according to the shape of the aperture (rectangular or circular) and the regularity or irregularity in the size of the grains. For a circular aperture and fairly regular distribution the mean value of $m = 0·30$ may be taken.

The time of exposure for a planetary surface will vary inversely as β^2 and s, the sensitiveness of the plate, or

$$T = \frac{k}{s}\left(\frac{e}{\lambda}\right)^2.$$

The diameter of the silver grains, e, varies with the sensitiveness of the plate, e increasing as s increases. No very exact data respecting the relation between e and s is available, but for the plates at present made the sensitiveness in general increases more rapidly than the square of the diameter of the silver grains. When time of exposure is a consideration it is therefore an advantage to use the fastest possible plates with smaller values of β (on account of larger values of e) rather than the reverse arrangement.

With the fastest bromide of silver plates the value of e would seem to be between 0·005 mm. and 0·015 mm. (see previous paper, also 'Lick Observatory Publications,' vol. iii. p. 12). Assuming the photographic objective to have been corrected for minimum focus at G(λ=4307 tenth-meters), we find for β (assuming mean value 0·01 for e):

$$\beta = 0{\cdot}013 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

or the linear aperture should be about $\frac{1}{80}$ the focal length of the objective. For finer-grained plates the ratio of aperture to focal length would be larger ($\frac{1}{40}$ for $e = 0{\cdot}005$), for coarser-grained smaller ($\frac{1}{120}$ for $e = 0{\cdot}015$).

It is rather important, I think, that the ratio of focal length to aperture defined by (1) be adhered to whatever the size of the instrument used. If the angular aperture β is made larger than is called for by this relation, we increase not only the cost of the instrument, but also the difficulties of using it, on account of the greater effect of atmospheric disturbances, and gain nothing except some slight increase in the brightness of the image, and therefore, other things being equal, a decrease in the time of exposure. If, on the other hand, it is made smaller, the time of exposure is increased unduly. The case is better put, perhaps, by starting with an objective of a given linear aperture, and considering what should be the focal length to obtain the best results. It is evident that in order to do full justice to the photographic method the focal length should be great enough to obtain full photographic resolution—*i. e.*, it should be at least equal to

$$\frac{ae}{m\lambda},$$

for if less than this the details which the objective of aperture, a, is capable of showing cannot be fully resolved on the photographic plate, on account of the finite size of the silver grains. On the other hand, if greater than this the time of exposure is increased without a compensating gain in any other direction. The size of the image is increased, it is true; but that is no advantage, for it was large enough before to show all the details which the objective itself is capable of showing. Mere size can be attained best by subsequent enlargement.

In previous experiments in photographing the planets a greater ratio of aperture to focal length than is here advocated has generally been used. Thus Professor Schaeberle seems to have used a ratio of 1 to 220 ('Popular Astronomy,' Feb. 1896), Holden and Campbell a ratio of 1 to 140 (the data in regard to the free aperture is not given in their paper, M. N. vol. lii. p. 499; and I assume it to have been 33 inches, the full aperture of the photographic objective), and the Greenwich observers a ratio of about 1 to 150 (estimated from approximate data furnished by Professor Barnard). I have not been able to find any data in regard to the enlargement used by Mr. Pickering in taking the photographs which he obtained of Jupiter and Saturn at Wilson's Peak in 1888–89 (when will that happy day come when observers will give full data in regard to their photographs?), but, to judge from the appearance of the reproductions ('Knowledge,' Nov. 1889, p. 253) and from a positive on glass in the possession of Professor Barnard, it could not have been less than 1 to 200 to 1 to 250. In all of these cases this unusual ratio of focal length to aperture was obtained by direct enlargement of the focal image by a second lens or mirror. I believe the use of this very large ratio heretofore has been a mistake, for the reasons already given. Professors Holden and Campbell are the only persons, so far as I can find, who have advocated the use of larger angular apertures than those given above. In their papers, already referred to, they state that the enlargement they used was too great to have secured the best results, and in a subsequent paper Professor Holden states ('Lick Observatory Publications,' vol. iii. p. 5) that a five-fold enlargement (giving a ratio of 1 to 85) gives much better results. This statement, together with the fact that their negatives are certainly as good as anything so far obtained, is an additional argument in favour of increasing the value of β to the limit indicated by (1). Against direct enlargement at the focus a further objection may be urged on the ground that it introduces an additional source of distortion and vibration, as well as an additional loss of light, as the lens or mirror which is used for the purpose of enlargement must have faces which are necessarily of rather short radius of curvature. For these and other reasons MM. Loewy and Puiseux have, after a long series of experiments, abandoned the use of an enlarging-lens in their work in lunar photography. The results which they now obtain by photographing the image at the primary focus (ratio of aperture to focal length about 1 to 30) are admitted to be just as good if not better than those obtained with the Lick telescope, in which in some cases an enlargement of about five diameters (ratio of aperture, 8 inches, to equivalent focal length about 1 to 350) has been used (Holden, 'Publications A. S. P.,' vol. viii. p. 319). No details in regard to the diameter of the silver grains in the plates used by the Paris astronomers have, so far as I know, been published. With plates of the fineness of grain of those used by Professor Holden (e=0·005 mm.), a ratio of aperture to focal length of 1 to 40 would have given full photographic resolution.

F. L. O. Wadsworth.

Yerkes Observatory.

[To be continued.]

The Great Meteoric Shower of November.

[Continued from p. 310.]

Expected Phenomena at the ensuing Return of the Leonids.

Under this head it is intended to refer to the probable character of the showers which will occur during the following six or seven years. Our knowledge of this particular stream, though it is pretty reliable and better assured than that of certain other showers, is yet far from being complete. The periodic time of the main group of meteors is known and its associated comet has been seen, the apparent astronomical radiant-point has been accurately determined, and the mean height of the meteors, when visible in our atmosphere, has been found from a number of observations. But there are still some features which require much further practical investigation, and among these may be mentioned the visible duration of the shower, the character of the radiation, whether precise or diffuse, the mobility or fixity of the radiant-point, and the speed of the meteors. For the purpose of gathering really trustworthy evidence as to these features, long-continued and very exact observations by well-trained eyes are necessary. A good deal was accomplished at the last return in 1866, and much of it was due to the efforts of the Luminous Meteor Committee of the British Association. The Committee alluded to suspended its labours some years since, but this branch has plenty of efficient observers, and among them are fortunately several of the well-known men who witnessed the display of a generation ago, namely, Prof. A. S. Herschel, Messrs. T. W. Backhouse, J. E. Clark, E. J. Lowe, W. H. Wood, and others.

In the case of a meteoric stream, there must necessarily exist some elements of uncertainty in reference to the exact character of its reappearances. It is not like a compact planetary body of limited dimensions, practically unchangeable form, and consistent motion. A meteoric system stretches over such an extensive space that its various sections are liable to be perturbed in very different degree by the planets passing near, so that it is often encountering vicissitudes of sufficient weight to change its distribution and form of orbit. Thus the Leonid showers of 1899 and bordering years may not furnish a counterpart to those which marked the epochs about 1833 and 1866, as considerable changes may possibly have operated since then to alter the visible aspect of the display. That such changes do occur is certain, but it is probable that they are rarely of sufficient magnitude to bring about the violent derangement of a system between successive returns. The changes probably have a more gradual influence in increasing the duration of the shower at each of its annual returns, and one result, chiefly of its physical construction, must be to lengthen out the group along the orbit so that at a distant period in the future the Leonids will be resolved into a similar shower to the Perseids of August, and exhibit no very brilliant maxima, but a regular annual shower of moderate intensity.

There is no substantial reason to apprehend that the Leonid displays now coming on will materially differ from those witnessed at the two preceding epochal returns. In every November until 1904 there will be prominent showers, more or less brilliant according to the varying astronomical conditions, to the character of the weather prevailing, to the position of the Moon, &c. The Earth passes through the really dense part of the cluster in a few hours, and if the *rencontre* should occur in daylight the stream may pass unheeded, or moonlight may overpower it, as it seems likely to do in 1899.

As stated on p. 196, Mr. B. V. Marsh, of Philadelphia, U.S.A., concluded from the evidence furnished by the showers of 1865–8 that there were really three branch streams, the denser central group of meteors being flanked on either side by lateral but wider and more tenuous clusters. The durations of these streams and intervening sparse regions he attributed as follows:—

Preceding group....	12 hours.	American observations 1865.
Tenuous space	6½ „	
Central group......	4½ „	European observations 1866, and American 1867.
Tenuous space	6 „	
Following group....	13½ „	Asiatic observations 1867, and American 1868.

It will be of great interest to gather new observations with the special view of testing the peculiar construction of the system adopted by Mr. Marsh in explanation of the various showers at and near the last maximum in 1866. The predicted times when the three groups, if really existing, will be centrally passed by the Earth are indicated in the following table:—

Computed reappearances of Marsh's Triple Shower.

	Preceding.		Central.		Following.	
		h		h		h
1896	Nov. 13	2¾	Nov. 13	17½	Nov. 14	8½
1897	13	9¼	14	0	14	15
1898	13	15¾	14	6½	14	21½
1899	13	22¼	14	13	15	4
1900	14	4¾	14	19½	15	10½
1901	14	11¼	15	2	15	17
1902	14	17¾	15	8½	15	23½
1903	15	0¼	15	15	16	6
1904	14	6¾	14	21½	15	12½
1905	14	13¼	15	4	15	19

The several showers discussed by Mr. Marsh were represented by him in a diagram given in the British Association Report for 1869, p. 303, and Prof. Herschel gave a similar figure representing expected showers to 1901 in 'Nature' of December 24, 1896, p. 175. I have constructed a diagram (p. 345) in different form but illustrating same features as the latter, and it is hoped this will give a graphic idea of the times of anticipated showers to 1905, while it also shows the extent of moonlight prevailing in each year at the important epoch. I have not endeavoured to picture the exact phase of our satellite in 1901 and 1904, for although she will be about 3½ and 8 days old respectively, she will be entirely absent from the sky after the Leonid radiant has well cleared the horizon. The black circle in these two instances represents, therefore, that the Moon will be quite invisible during the progress of the shower.

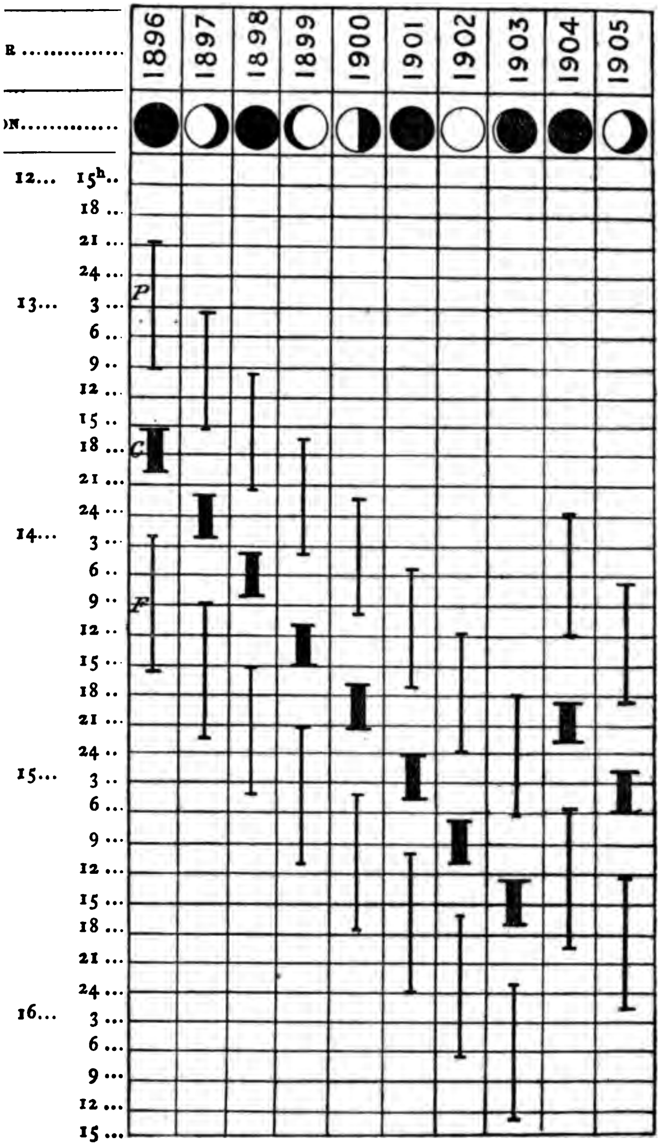
The years which apparently offer the best prospects in Europe are 1898, 1900, and 1901, as far as a dark sky is concerned, though 1900 is only partially favourable in this respect. But as regards the probable time of return of the richest section of the group we shall be most favoured in 1899, when the Earth should pass centrally through it on November 14 at 13^h. But the full Moon will then be shining in the sky. In speaking of the anticipated displays it may, however, be best accomplished by referring to the various years *seriatim*.

1897.

In 1864 (two years before the max. of 1866), a grand display of meteors was witnessed from the deck of a ship off Malta on the morning of November 13, and meteors were conspicuously numerous on the same morning in America. We are therefore entitled to expect a pretty abundant display in 1897, and should it prove a return of the same group which supplied the meteors of 1864, it will probably return in the morning hours of Nov. 14. But the Earth will intersect a point of the meteoric orbit rather further in advance of the comet than it did in 1864, and so the exhibition of 1897 is not likely to equal the former, unless, indeed, the richer part of the stream has lengthened out in the interval of 33 years, which is quite possible. The really dense part of the system which furnished the brilliant European display of 1866 (max. Nov. 13, 13^h 10^m), and the equally imposing American shower of 1867 (max. Nov. 13, $22\frac{1}{2}^h$), is not likely to be visible in England, as the Earth will be centrally involved with it on the 14th at midday. In America something of it may possibly be seen just before sunrise on the 14th. The Moon will be full on the morning of the 9th, and will therefore be gibbous on 14th, rising at 7^h 55^m at Greenwich, and being visible in Gemini during the whole night afterwards. The prospects are therefore by no means good, but a close watch should be maintained on the mornings of the 14th, 15th, and 16th, for if the atmosphere be clear, meteors will sure to be frequent, and fine ones may appear every now and then.

1898.

This return will be more auspicious in several respects than that of 1897. The Moon being new on the morning of Nov. 14, there will be no objectionable light from that orb. Apart from this circumstance we shall be a year nearer the max., and probably within one year of it, which should make all the difference by considerably augmenting the number of visible meteors. In 1865, which nearly corresponds with 1898, though it was a little more


R ....
ON............
1896
1897
1898
1899
1900
1901
1902
1903
1904
1905
12... 15h..
18 ...
21 ...
24 ...
13... 3 ...
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favourable, according to one description "the heavens were in a blaze with shooting stars" on the morning of Nov. 13, and at Greenwich 279 meteors were seen on the same morning. These facts, and the suggestive proximity of the max. in 1899 or 1900, sufficiently indicate that a plentiful shower will appear probably on the mornings of both the 14th and 15th. The group of 1866 is not likely to be seen in this country, as the Earth passes through it on the early evening of the 14th, and some hours before the radiant will have risen. At places far east something may be seen of it where the radiant rises before 9^h G.M.T.

1899.

Both favourable and unfavourable conditions will influence the character of this return. The meteors ought to form a splendidly abundant shower in England, for the central and densest group is likely to encounter the Earth on Nov. 14 at 13^h, when the radiant will be fairly high, as at the time of max. in 1866. Most unfortunately, however, the Moon will be nearly full and visible during the whole night. But if the sky is clear it is certain that a large number of meteors will be observed. The moonlight will certainly obliterate many of the fainter objects and rob the brighter ones of much of their brilliant appearance, which is only fully realized when a dark sky affords its useful contrast. The appearance of a fine meteor is not startling so much on account of its size or suddenness as for the instantaneous manner in which it lights up a sky and landscape previously involved in darkness. With the rays of a nearly full Moon illuminating the prospect on Nov. 14, 1899, the flashing of large Leonids will quite lose its effect, and, indeed, the whole display must suffer serious detraction. From the same cause the streaks left by the meteors will appear faint, and in many cases will escape detection altogether. There will also be greater difficulty in accurately marking down the tracks of the individual meteors, as only tolerably bright stars will be visible. But in spite of these drawbacks the shower should be attentively watched and its strength determined as carefully as the conditions allow. It is probable that it will fully repay every effort made to witness it.

1900.

At this return the Moon will be near last quarter, and rises at about midnight. Her influence upon the shower will not therefore be serious, and as we shall be immersed in the richest part of the stream just before sunrise on Nov. 15, a brilliant display will probably be observed in England. The number of meteors furnished by this system is likely to be quite as great in this year as last, and when all the circumstances affecting the visible nature of the phenomena are considered, the return of 1900 appears to give promise of greater importance than that of 1899. It will be remembered that the display of 1867 was a magnificent one as observed in America, though the full Moon moderated it in a large degree.

1901.

In this year also the meteors are likely to be exceedingly numerous and to form the third of a series of really first-class meteoric exhibitions such as were witnessed either in England or America in the three following years: 1866, 1867, and 1868. It is true that the central or richest group will probably encompass the Earth at about 2^h on Nov. 15 in England, so that neither in Europe or America is it likely to be seen to the best advantage. Remembering, however, that our data are not unimpeachable and may prove faulty in some of their indications, it will be well to look for a fine shower this year on the morning of the 15th. Should few meteors be seen at that time the watch should be renewed on the following morning. The sky, if clear of clouds, will be highly favourable this year, for the Moon being only a few days old at the important time, will have set before the Leonid meteors begin to display themselves.

1902.

Much of the importance of the displays marking the few previous years will probably be lost in this, for the Moon will be full and, apart from this, the portion of the meteoric orbit intersected by the Earth will be relatively poor in comparison to those sections much closer to the derivative comet. If the meteors are more numerously distended along the orbit than in former times, we may quite possibly witness a shower quite exceeding expectation. It is certain that both in 1836 and 1869 (3 years after the best returns) many Leonids were observed, though the numbers showed a great falling off as compared with the previous years. In 1902 we may have a return possibly equalling a tolerably active shower of Perseids. The best part of the stream ought to be manifested soon after the rising of the radiant on the night of Nov. 15.

1903.

On the morning of the 16th many meteors will probably be seen, and the firmament, if clear, will be dark, for the Moon will only be visible as a slender crescent for a short time before sunrise.

1904.

There will be no Moon to interfere with the display this year, for though our satellite will be near her first quarter, she sets before midnight, and will therefore be absent from the sky after the Leonid radiant is fairly displayed. Leap-year will throw the shower nearly a day back this year, and it is likely to be best seen on morning of Nov. 15. It was well seen in 1838 on morning of Nov. 13.

1905.

The Moon will be gibbous and visible all the time. This circumstance, coupled with the fact that the comet and all the richest part of the system will now have receded far from the Earth and a long distance on its journey to beyond the orbit of Uranus, will probably cause the shower to present a very insignificant display. W. F. Denning.

[To be continued.]

The Unification of Astronomical Constants.

The resolution passed at the Paris Astronomical Conference of 1896, that the fundamental constants of astronomy used in the computation of the four national ephemerides should be the same from the beginning of the next century, is not meeting with universal approval. Discussion of the subject has been opened in the 'Astronomical Journal,' No. 410, where Prof. Boss and Dr. Chandler both give expression to their views. Prof. Newcomb's determination of the constant of precession gives Prof. Boss the keynote for his article. He discusses this technically, and shows that though Prof. Newcomb has treated the data used by him with his usual logical skill, the result cannot be accepted as final, and urges that the time is not yet come when we can adopt values for this constant and for the constant of aberration which may not have to be changed in the course of the next half-century. He thinks that there is no value of these constants which would be accepted by the astronomical public with the same confidence as is the value 8″·80 for the solar parallax. The following extracts from Prof. Boss's article are illustrative of his views:—

"Doubtless Professor Newcomb has sufficiently exhausted the consideration of the evidence as to the true value of the precession-constant which may be derived from the comparison of Bradley's star-places with those of Greenwich for the mean epoch 1865; but it seems to me that this is only a small part of the evidence which ought to be considered. To call upon astronomers to endorse the constant of precession which Prof. Newcomb has found in this way, appears to me almost as unreasonable as it would be to ask them to adopt a value of the constant of solar parallax derived from a partial discussion of the material available through a single method. The method and material selected might be of the best, and the result might be near the truth; but there would certainly be lacking the essential element necessary to impart confidence in the result. How much such a determination may be in error, though emanating from the most competent authority, and received with universal confidence, is illustrated in the history of Encke's determination of the solar parallax.

"No great inconvenience can arise from the retention of the Struve-Peters constants for a few years longer, until we are in a better position to agree upon some value of those constants likely to satisfy astronomers for a considerable term of years. The difference between the aberration-constant in present use and that proposed by the Conference is only 0″·025; the corresponding difference for the precession-constant is 0″·008; and, for the constant of nutation, the difference is 0″·014. These proposed changes are practically insignificant in relation to all observations of a differential character; and, even if we grant that they are the actual corrections required, no serious harm can result if the use of them be deferred, since it is morally certain that astronomers will not rest satisfied with the present state of our knowledge in reference to the constants of precession and aberration."

Dr. Chandler's article, written in support of Prof. Boss, is not too long for us to transcribe here in full. We shall be glad to publish the opinion of any English astronomers on the subject, the expression of which Dr. Chandler thinks is so desirable:—

"The contemplated change in the values of the astronomical constants to be used by the various national astronomical ephemerides, beginning with 1901, has naturally excited lively interest, and aroused much discussion which, so far, has not reached public expression. That the matter could not fail to reach the stage of open discussion, at some time or other, is a matter of course. The thanks of astronomers are due to Mr. Boss for his dispassionate, courageous, and forcible presentation of the subject in the foregoing article.

"But more than this silent approval seems to the writer to be desirable. The situation is one that calls for the individual expressions of opinion by astronomers generally. The discussion is forced upon them both by the importance of the subject, and by the singular features of the method by which the proposed changes have been decided upon. No apology, therefore, is needed from any one for engaging in it; on the contrary, it is an incumbent duty upon all who have an effective word to speak with regard to the proposition, whether of antipathy, apathy, or sympathy.

"It would seem to be indisputable that any change of this kind is so important and entails so much inconvenience that it should not be undertaken except after full debate and the general assent of competent opinion. Nor should it be made until the data upon which such opinion must rest have been brought up to within a reasonable time of the date when such change is to go into operation, and are of such a character as to make it likely that the values determined upon will serve for the greater part of the coming century. Without here going into a technical argument, it certainly does not seem that this state of things now exists. Furthermore, there is no good reason whatever why such an alteration should begin with the twentieth century. Surely the making so serious an act contingent on the sentimental association vulgarly attaching to round figures, cannot be gravely considered by scientific men; nor can any adequate argument be advanced for the greater convenience of the date in question, in preference to any other.

"It is further to be apprehended, indeed it may be looked upon as reasonably probable, that if the changes are now effected as proposed, they will prove insufficient before a considerable portion of the century shall have elapsed. In such case the consideration of a second change will then have to be entertained. Such a contingency threatens so much inconvenience that it ought, by all prudent foresight, to be avoided.

"Therefore, while admitting that an alteration of the constants at present in use will become desirable, and can advantageously be made under proper conditions of knowledge as to what they should be, perhaps within a decade, the writer desires to express a conviction that the adoption of the change proposed, under the present conditions, is premature and unwise. On the other hand he can see no disadvantage in its postponement.

"In any event it seems to be desirable that some one, at least, of the National Almanacs should for the present retain the old constants, for the purpose of serving the large body of astronomers the world over who will prefer to use them in their investigations, until a more definitive change can be generally made. In default of this, the suggestion of Mr. Boss at the conclusion of his article, is a practical and feasible one; namely, that some action be taken—as it is pretty sure to be taken—looking to the provision of such facilities by astronomers for themselves, independently of the almanacs, which may then disappear from the astronomers' book-shelves, and be relegated to ship-cabins, where the question of the basis of their construction is a perfectly immaterial one."

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Tycho Brahé and Astrology.

GENTLEMEN,—

No doubt there have been some remarkable lucky hits in so-called astrological predictions, but there is no occasion, by looseness of writing, to make these seem more remarkable than they really are; if truly predictions, in the ordinary sense, they would of course agree with the facts in every detail. I was led into this reflection by reading the article on "Astrology" in the ninth edition of the 'Encyclopædia Britannica'; and as it is to be presumed that a tenth will soon be commenced in which Astrology and Astronomy will necessarily occupy an early place, that is an additional reason for pointing out the error in question. "But besides this" (we read, p. 742) "we find him [Tycho Brahé] in his character of astrologer drawing a singular prediction from the appearance of this comet [meaning the comet of 1577; but the prediction really was made in reference to the Nova Cassiopeiæ of 1572, and connected also with the planetary conjunction of 1583]. It announced, he tells us, that in the north, in Finland, there should be born a prince who should lay waste Germany and vanish in 1632. Gustavus Adolphus, it is well known, was born in Finland, overran Germany, and died in 1632."

Now, so far from its being well known that Gustavus Adolphus was born in Finland, his native place, it need hardly be pointed out, was Stockholm. Undoubtedly, as Dr. Dreyer remarks in his admirable and exhaustive account of Tycho Brahé, Finland was then included in the kingdom of Sweden, and Finnish regiments followed Gustavus in his German campaigns; but Tycho was evidently thinking of some Russian commander, whose birthplace was to be "in Russia or Moschovia, where it joins the north-eastern part of Finland," about latitude 61¾°, in the zenith of which he supposed the Nova to have been when it first appeared. Moreover it would seem that he considered the great prince in question would be born about the time when the star appeared; Gustavus was not born until twenty-two years afterwards, in 1594. Russia, at the time of which we are speaking, was still under the rule of Ivan the Terrible.

Yours faithfully,

Blackheath, 1897, Aug. 7. W. T. LYNN.

The Metric System.

GENTLEMEN,—

Mr. Ellis's letter in your current number induces me to send you some remarks on the Metric System.

Was the use of fractions forbidden by the authors of the system? I do not believe it was: in fact the half of a unit was distinctly spoken of as being equivalent to 5 of the subordinate unit. What was determined was that, save this fraction no other should have physical representation in the system of weights, measures, and coins. If teachers neglect to use their common sense in teaching simple mental arithmetic, the fault is hardly to be attributed to the system.

We all know that the lawmakers, who enacted the use of the System, were not free from making somewhat extravagant claims, and probably in those times any calm discussion was an impossibility. Are any advocates of the system now so bigoted as to desire that we should adopt the defects as well as the merits? Does anyone desire to see our pound sterling abandoned for that franc which was loudly declared to be the measure of value for ever?—a franc which probably has now no existence out of a museum.

But there is another point of view which opponents of the Metric System do not always see, even the ablest of them. It is that the problems which offer difficulty often do so because they are stated in our units. In the case mentioned in Mr. Ellis's letter* this is to some extent true—the problem which would have been offered to the shopman in this case would have been 1·6 metres of ribbon at 16 mils (thousandths of a pound) per metre, which is not so difficult. I was much struck, in reading a Blue-book, by finding a very eminent authority express his difficulty in ordering a table 30 inches high on the metric system. Of course he completely forgot that, subject to the probable errors of a carpenter's work, it was expressed by 76 centimetres. Probably no one now would talk of the necessity of an infinite, or even a large number of decimals to convert any measure for an artificer from our units to those of the metric system; hardly any physicist would complain of the figures requisite to denote his constants.

As regards the difficulty of dealing with our money, I think it is imaginary: it is so easy to convert decimals of a pound into our money with all needful accuracy, that I cannot think anyone could have the least trouble in keeping books in decimals while making payment and receiving in our currency—*that* and not the metric system is what is wanted in teaching at first: appreciation of the units can only come by practice; it is not to be learnt from books. I speak with some confidence, for I often make calculations in this way so as to use a slide-rule. No one proposes to change the pound sterling.

Of course it is easy for an opponent to put forward posers: we all know that the perplexing questions of examiners in arithmetic never are seen in practice, even with our extraordinary want of system in our units of quantity and value: that is simply because common sense evades them; and it is certain that the same thing would result when the metric system was in use. Of course I do not expect to meet the circumstances of children who buy a quarter of a yard of ribbon at $\frac{3}{4}$*d.* for their dolls' hats, and expect some wonderful denomination of change. There the solution would be probably, as now, to the advantage of the small customer.

If I believed in the use of argument in such a matter I should probably join some society for the promotion of the system; but I do not. The only practical measure I can think of is to induce one of the big Stores to import French assistants†, and use Metric Weights and Measures, under the Permissive Act, with decimals of the Pound as money. Fashion would probably make the experiment a great success, and the example would spread; meanwhile all the bogies would come to a natural end.

Maida Hill, W.
1897, August 4.

Yours very sincerely,
J. F. Tennant.

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* 1$\frac{3}{4}$ yards at 3$\frac{1}{2}$*d.*

† P.S.—Of course the *language* of the Stores would be French, or the English of Paris! Any attempts to acquire an English accent by the assistants would be severely repressed!

Personal Equation: and the Amateur Astronomer.

Gentlemen,—

While quite ready to say with Cicero, "Non est meum contra auctoritatem senatûs dicere," I really must express my dissent from the appraisement of the value of Professor Safford's paper on Personal Equation by the owner of the Oxford Note-Book. Under existing circumstances, Personal Equation enters as a disturbing element into nearly every description of observation, and its elimination, photographically or otherwise, could and would only prove a real and incalculable benefit alike to the observer and the fundamental astronomer. This being so, I rather fail to see why it should be pooh-pooh'd and relegated to the dilettante. As to the extent of the interest attaching to this subject, I, in my weak-minded way, would humbly venture to submit that it is at least equal to that of the R – D correction, or even of the measurement of the plates taken for the Astrographic Catalogue, which appears to be the "King Charles's Head" of every Report issued from certain observatories for some time past, but concerning which a very large proportion indeed of the mere "astronomical public" "do not," in the emphatic phrase of the penultimate Duke of Bedford, "care one twopenny d——."

The astronomical amateur is, I am painfully aware, but a poor creature, who should be but too grateful for the crumbs which fall from the rich man's table of the subsidized public observatory; but he is still human, and, in some sort (as they used to say on the anti-slavery tracts), "a man and a brother." As it is, there is, I fear (or seems to be), a disposition on the part of the professional astronomer to treat him somewhat after the fashion in which a well-known Q.C. was patronizing a couple of Her Majesty's judges in the High Court of Justice, until checked by one of them exclaiming, "Mr. So-and-so, you really address this Court like the Almighty talking to two black-beetles!"

Forest Lodge, Maresfield, Uckfield.
1897, August 3.

Yours faithfully,
William Noble.

[Is Captain Noble quite fair? I by no means intended to pooh-pooh "personal equation"; but I said that Prof. Safford did not seem to me to contribute anything new on the subject. Nor did I refer to the astronomical amateur, but to those astronomers, whether amateur or professional, whose knowledge is not very extensive. Some of those who know most are amateurs. By the way, are amateurs the poor, and officers of the subsidized public observatory the rich? I thought it was the other way.—The Owner of the Note-Book.]

Proctor's (?) Menacing Comet.

Gentlemen,—

I think that the writer of the very amusing series of paragraphs which appear monthly in your columns "From an Oxford Note-Book" will find that the late Mr. R. A. Proctor wrote an article in an Australian journal (subsequently reproduced in 1882 in his 'Familiar Science Essays') on the possible identity of the Great Southern Comet (I.) of 1880 with the famous one (I.) of 1843; and that it was the 'Spectator' which, misinterpreting what he there said, attributed to him the prediction of the vaporization of mundane things in general 15 years later, or in 1897. For Mr. Proctor's repudiation of any such prophecy, the notetaker may refer to vol. i. of 'Knowledge,' pp. 320 and 340. *Vide* also pp. 404 and 450 of the same volume.

Forest Lodge, Maresfield, Uckfield, Yours faithfully,
1897, August 4. William Noble.

[I am much obliged to Capt. Noble, and also to Mr. Lynn, who supplied similar information.—The Owner of the Note-Book.]

The Counting of Clock-beats in Transit Observations.

Gentlemen,—

I have been wondering lately who could have been the "Greenwich Observer" referred to in the interesting little anecdote inserted on p. 218 of the present volume of the 'Observatory,' which tells of an occasion when Le Verrier asked a certain person to count clock-beats from his inner consciousness.

My old friend and colleague, Mr. Ellis, also refers to this story in his very interesting reminiscences on p. 313, but he remarks: "which story I do not remember to have heard before, though probably I have, since the observer in question would be an old colleague of mine."

Now all this very pretty little reminiscence is quite new to me. It so happens that I have had a life-long acquaintance with this "Greenwich Observer," who was the private guest of Le Verrier at the Paris Observatory during the first part of the longitude operations in May and June, 1854, and therefore could hardly fail to be acquainted with the fact of such an interesting experiment as that referred to in "The Oxford Note-Book," had it ever taken place. I am assured that the "Greenwich Observer" still retains the most delightful remembrances of his visit, and of the many conversations, astronomical and otherwise, that he had had with Le Verrier and the Paris astronomers; but he cannot recall to his memory any circumstance which might have reference to this alleged trial of his skill in counting clock-beats out of hearing of any clock. One thing he is certain of, that Le Verrier never showed any signs of "disgust" in his presence, except upon the occasional unaccountable failures of the galvanic signals. Probably the story originated from some slight remark of Le Verrier, and was magnified in conversation long after the event.

I have read Mr. Ellis's reminiscences with great interest. They carry my thoughts back fifty years or more, and to the time when transit observing was altogether made by the eye-and-ear method. My experiences of transit observing at that time are very similar to his, and his explanation of his method of estimating the times when the object passed the separate wires coincides exactly with that adopted by myself before the introduction of the chronographic method of recording transits in 1854. There can be little doubt that the principal transit observers, from their long experience of observing by the eye-and-ear method, had the power of estimating small fractions of a second with great accuracy. Le Verrier, during the longitude operations, often expressed a feeling against the chronographic method then just adopted at Greenwich, and he has been known to remark that "Cela fait de mauvais astronomes" (M. N. vol. xxiv. p. 159). However, I think that there are few astronomers of the present day who would care to revert altogether to the observation of transits by eye-and-ear.

27 Kidbrooke Park Road,
Blackheath, S.E.
1897, August 2.

Yours faithfully,
EDWIN DUNKIN.

[In view of Mr. Dunkin's evidence, which is of even greater value than his modesty will allow him to indicate, the story about counting the clock-beats must be added to the category of burst bubbles. I trust he will accept my apologies, though I have not the skill of Mr. Labouchere in executing these strategic movements to the rear.—THE OWNER OF THE NOTE-BOOK.]

OBSERVATORIES.

NATAL.—The Report of Mr. E. Neville Nevill, Government Astronomer of the Colony of Natal, is remarkable as a Report, but interesting since it contains an outline in popular language of the lunar theories of Hansen and Delaunay, leading up to an explanation, that the tables of neither astronomer can be regarded as perfect. Mr. Nevill goes on to point out that for the completion of the Lunar Theory two things must be done. One is "the reduction of all the observations of the Moon to a uniform basis and their comparison with the portion of Hansen's tables whose theoretical accuracy is undoubted. Then from the difference between the two results there must be determined the approximate value and the probable period and character of all the terms of long period and their associated terms of shorter period which are indicated by the observations, and shown by the comparison to be required to be added to the known portion of the complete theory."

"The other is to devise a simpler and more powerful method of calculating from theory the value of the terms of this kind and to apply it to the computation of the exact values of all the terms shown to exist by the comparison between theory and observation."

Researches such as these have been in progress at the Natal Observatory for some years. Mr. Nevill has completed the comparison with Hansen's Tables of about 15,000 observations of the Moon made at Greenwich from 1750 to the present time, supplemented by others made between 1620 and 1750, reduced to a uniform basis. The second research is far advanced, but requires much time and labour to be spent on it to put it in a state fit for publication. This valuable excursus on the lunar theory is laid before the authorities as a reason for the necessity of including these researches in a volume of Natal Observations, which should be published shortly and the preparation for press of which has been the main occupation of the Director and his Senior Assistant (Mrs. Nevill) during the year 1896.

This and other work on the arrears of reduction and tabulation have left little time for astronomical observations beyond those necessary for the time service. The work of reduction now in hand is a continuation of that reported last year, namely, the comparison of declinations of stars made at observatories in the northern and southern hemispheres (Talcott's method), and a few observations have been made in connection with this.

The staff at the Natal Observatory consists of Mr. and Mrs. Nevill and Mr. F. A. Hammond, who is the Meteorological Assistant. The post of Junior Assistant has been vacant since 1896 September 1, and the Director writes of the difficulty he has in obtaining properly qualified assistants. This difficulty is similarly felt by the Directors of other Colonial Observatories; the salary on beginning is not sufficiently large to tempt a man who has the necessary experience. Such a person as can be got for the price is of little value for a year or so, and when he has acquired the experience he generally finds a post in some other occupation where the salary is a little higher and the chances of promotion are greater. The remedy for this is fairly obvious. Governments and others who keep observatories should be taught to realize that the work of an astronomer is at least as arduous and as much deserving of reward as that of a clerk in more routine departments.

Bound up with the Report are the meteorological observations made on every day during the year, with a summary, and also a summary from other stations in the colony. Among the phenomena calling for notice was the high temperature on Sept. 21 and Oct. 3. September 21 is said to be the hottest day ever recorded at the Observatory. By half-past 10 o'clock the temperature was over 100°, and by 2 o'clock the thermometer registered 109°·6.

NOTES.

Comet Notes.—The following ephemeris of d'Arrest's Comet (for Paris midnight) is given by M. Leveau in Ast. Nach. 3441:—

	R.A.	N. Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
Sept. 6....	4 22 48	1 57	Sept. 22....	4 32 31	0 41
10....	4 26 11	1 20	26....	4 33 20	1 22
14....	4 28 56	0 41	30....	4 33 29	2 4
18....	4 31 4	0 0	Oct. 4....	4 32 58	2 45

The comet is exceedingly faint, and steadily though slowly diminishing in brightness.

Mr. Merfield communicates to Ast. Nach. 3438 the following parabolic elements of Comet 1897 I. (Perrine), deduced from ten observations made in Europe in November and December, and nine observations made by Mr. Tebbutt in March and April:—

T = 1897 Feb. 8·0816 G.M.T.

ω	172° 17′ 39″	1897·0.
☊	86 28 31	
i	146 8 44	
$\log q$	0·02634	

Miss Anne S. Young communicates to Ast. Journ. 408 the following elliptic elements of Comet 1896 VII. (Perrine), based on observations extending from the beginning of December to the end of January:—

Epoch = 1896, Dec. 11·5.

M	2° 32′ 32″	
☊	246 32 59	1897·0.
ω	163 56 26	
π	50 29 25	
i	13 41 26	
$\log e$	9·83377	
$\log a$	0·54309	
$\log \mu$	2·73537	

A. C. D. C.

MINOR PLANET NOTES.—The following two planets have received permanent numbers (Ast. Nach. 3436):—

Provisional Designation.	Date of Discovery.	Discoverer.	Number.
DB	1896 Dec. 7	Charlois	423
DF	1896 Dec. 31	Charlois	424

DC, DD, DE do not receive numbers, not having been sufficiently observed.

A. C. D. C.

OBITUARY.—ALBERT MARTH. The ravages that death has recently made among astronomers of all classes has been already commented on. We now have to regret the death (on August 5, at Heidelberg) of Mr. Marth, who had a world-wide reputation as *the* astronomical computer *par excellence.*

Albert Marth was born at Colberg, in Pomerania, on May 5, 1828. He was educated at Berlin and Königsberg, where he commenced his astronomical labours under the late Prof. C. A. F. Peters, afterwards editor of the 'Astronomische Nachrichten.' In 1853 he came to England and was for a time Assistant at Mr. Bishop's observatory in Regent's Park, where he discovered the small planet Amphitrite in 1854. His next post was at the

Durham University Observatory, which he occupied from 1855 to 1862, afterwards spending two years assisting Lassell in his observations, chiefly of satellites and nebulæ, at Malta. Subsequently he held no fixed office until 1882, when he took charge of Col. Cooper's observatory at Markree Castle, co. Sligo. For many years Mr. Marth has contributed to the 'Monthly Notices' valuable ephemerides of the satellites of the planets and of the surface conformations of Mars and Jupiter; and as illustrative of the comprehensive command that Mr. Marth had of such matters it may be sufficient to mention that he contributed a few years ago to the Royal Astronomical Society a paper "On the Transit of Mercury over the Sun's Disk, which takes place for Venus on 1894 March 21, and on the Transits of Venus and Mercury which occur for Saturn's System on the same day."

In any notice of Mr. Marth and his work it is impossible to omit some reference to his adverse criticism of the Greenwich methods as applied to observations with the transit-circle. This was contained in a paper entitled "On the Polar Distances of the Greenwich Transit-circle," which was communicated to the Royal Astronomical Society in 1860, but was rejected by the Council as unsuited to the 'Monthly Notices,' and afterwards appeared in the 'Astronomische Nachrichten,' Nos. 1260–3. A number of minute points of detail were gone into, in which it was thought to be shown that inaccuracies of adjustment were not sufficiently corrected. The Astronomer Royal (Airy) was annoyed at the appearance of the paper, but made no public reply to it, though some investigations were undertaken, which were considered to prove that Mr. Marth's criticisms were uncalled for; and the only change made in the Greenwich methods of reduction in consequence was that the fractional parts of the proper motions of the stars in which this could be considered a known quantity were applied with the star-corrections in bringing the places to the beginning of the year. ....

The honorary degree of M.A. was conferred upon Mr. Marth by the University of Durham in 1857; his Fellowship of the Royal Astronomical Society dates from January 1878.

MR. S. E. PEAL. We also regret to announce the death of Mr. S. E. Peal, of Assam, who has been the author of several astronomical papers, dealing in particular with the physical characteristics of the Moon.

VARIATION OF LATITUDE.—In 'Astronomical Journal,' No. 406, Dr. Chandler publishes a "Synthetical Statement of the Theory of the Polar Motion." There are no essentially new facts in this, but assuming the rotation of the pole, according to the two well-known component motions which he has already found by analysis of observations, he develops from this the geometry and formulæ which give the variation of latitude, azimuth, and longitude at any place. The formulæ and notation correspond exactly to those contained in previously-published articles, and therefore the number will be found useful to anyone studying this interesting subject.

THE YERKES OBSERVATORY.—We hear that the opening ceremony of this Observatory, and the series of conferences connected therewith, have been postponed owing to the personal arrangements of Mr. Yerkes, who will not return from Europe until Oct. 16. The limiting dates originally fixed were Sept. 27 and Oct. 1, but these have now been altered to Oct. 18 and Oct. 22, both days included. The provisional programme is most comprehensive, and too long to give in full, but it may be summed up by saying that in the daytime addresses will be given on astronomical, spectroscopic, astrophysical, and electrical subjects by members of the Staff of the Observatory, and also by distinguished visitors, such as Comstock, Doolittle, Laves, Pickering, and Pritchett, on subjects with which their names are especially connected. There will be demonstrations in the optical and mechanical workshops by practical men, such as Brashear, Lundin, and Mors, and in the evening hours the 40-inch telescope (we are informed that the rising floor will be restored by then) will be used by Profs. Barnard, Wadsworth, and Hale to show the wonders of the heavens. The formal presentation by Mr. Yerkes, and the acceptance by the President of Chicago University, is fixed for 11 o'clock A.M. on Oct. 21, and the ceremonies are to be brought to a close by a banquet on Friday, October 22.

ASTRONOMERS: AMATEUR AND PROFESSIONAL.—We publish in this number a letter from a breezy correspondent of ours which almost compels us to take up the cudgels on behalf of the poor persecuted professional astronomer. Not that we take our gallant correspondent too seriously, for we know very well that there is no man alive who can appreciate the value of a good star-catalogue better than he; but we prefer to take the first part of his letter to mean that, in his opinion, domestic details such as small corrections, however necessary they may be to accuracy, should not be so much flaunted in public. We think that on consideration our correspondent will see it is just because the star-catalogue maker is subsidized from the public purse that he feels that these details, some of which relate to matters of controversy, should be submitted to public criticism before their final adoption.

The second part of his letter raises a different question, namely, the relation between amateur and professional astronomers. There is no doubt but that the gulf between these two classes has for some years been widening. Time was when leading amateurs, Groombridge, Carrington, and Baily, devoted themselves and their resources to the making of star-catalogues, but there were few such men who were able to devote the many years necessary for making a record long enough to be of any value. Since their time an army of amateurs has arisen who make a hobby of a science which certainly has the same name but otherwise is quite distinct from that at which these men worked, and the older science of celestial surveying is carried on, as necessarily it must be if its results are to be of value, by state-supported establishments which exist and endure independently of any man's life. It is as inconsequent to make comparison of results achieved by these two classes of workers as it would be to compare the results of geology and geodesy. There is no one in England who is paid to study the lunar surface, the markings on Jupiter and Saturn, or the variation of the light of stars; and hence we fail to appreciate the argument of a recent correspondent (Fellow of the Royal Astronomical Society) to a certain weekly publication devoted to amateurs, who mentions the work done by the late Mr. Elger, Mr. Green, Mr. Stanley Williams, and others in a way which, by contrast, casts a reflection on official astronomers.

We began this note by speaking of the professional as "persecuted"; and when writing this word we had this same publication in our mind, for we have seen in it at various times charges made against officials by these arrogant amateurs (we do not mean the F.R.A.S.), which would be serious if they were not made by persons who obviously knew nothing of the subject on which they were writing. We have seen suggestions made that nothing is ever published from Greenwich Observatory, that the time-signal from the same establishment is always in error (we forget whether it was said to be by a quarter of an hour or a day and a half), that the instruments are not being properly taken care of, and so on. These things are mentioned here as illustrating the light in which professionals are regarded in some quarters, and we are bound to say that we cannot recall instances of the "disposition" of the professional described on p. 353 by our correspondent. The moral seems to be "Live and let live"; but it should be distinctly remembered that national observatories are established for the accomplishment of particular kinds of work which are not to be changed according to the fashion of the hour, and that the work of the professional astronomer is such as does not allow him much evening leisure to go comet-finding or planet-gazing.

VISITORS AT GREENWICH.—The record shown by the visitors' book at the Royal Observatory generally receives some additions during the holiday months of July and August. This year we have been honoured especially by Oriental visitors. Heading the list comes the King of Siam, who took a very early opportunity after his arrival in England to visit Greenwich. The King, with Prince Chira and suite, drove down from town on the evening of Sunday, August 1, and were received by the Astronomer Royal, several members of his staff being in attendance. The evening promised to be fine, and preparations had been made to show the distinguished visitors some of the more remarkable celestial objects with the large instruments; but just before the Royal party drove into the park the sky clouded over and the only thing possible to be seen was Vega, and this was through thin cloud. So the visitors had to be content with seeing instruments and photographs, and after being entertained in the Astronomer Royal's house, went away apparently pleased with their visit.

We have also had at Greenwich recently the Raja of Rajputana; the Persian Minister in London; Count Roda of Germany, with some German officials; and some gentlemen from the Chinese Embassy, with students from Pekin.

HOWEVER gratifying it may be to feel that some interest is taken in our science, as shown by visits of the "dilettante" public to observatories, it cannot but be denied that the non-astronomer is a little trying. He has an idea that with a large instrument it is always possible to see stars, by day or by night, whether it be cloudy or not; and if it be possible to exhibit Vega or Pollux the visitor is always disappointed—he seems to imply, by his manner, that the small size of the image is a disgrace to any well-regulated observatory. It would not be difficult to make a collection of crooked questions and remarks by visitors sufficient to fill a small jest-book; but to spare our readers the proverbial dulness, we will be content with quoting the remark of the gentleman who, after having had a famous large telescope explained to him at some length, as he turned to leave the room, tapped the wooden tube and remarked: "I suppose this is *solid* right through?" This, which has been hitherto regarded as an extreme case, was almost equalled by a visitor at an exhibition where some lunar photographs taken with a very large telescope were being shown, who took up the magnifying lens placed for the purpose of examination with the remark: "And is this the instrument they were taken with?"

THE following appeared in the 'Times' of Aug 5.:—"A Reuter telegram from Berlin of yesterday's date says: 'According to a communication made by the Royal Observatory, Prof. Goldstein, of this city, has succeeded in experimentally reproducing, by means of kathode rays, certain very distinct and characteristic cometic phenomena, such as the radiation of light from the head of a comet and the resultant development of a tail. He has also been able by these means to account for certain peculiarities of this class of phenomena which have been observed in recent years.'"

WE regret to see announced the death of Col. C. F. Crocker, one of the Regents of the Lick Observatory, a liberal patron of astronomy, who provided the funds for the Lick Observatory expeditions to Cayenne (December 1889), to Japan (August 1896), and also for the proposed expedition to India.

OUR esteemed contributor Mr. W. T. Lynn has lately published a little book called the 'Penny Chronology,' and sold for that small sum. The book is not exactly astronomical, but nevertheless may be found useful by astronomers as by others.

FROM AN OXFORD NOTE-BOOK.

UNIFORMITY is very well in its way, but sometimes we seem to be in danger of getting a little too much of it. This text could doubtless be put into a much better literary form, but it expresses the main idea sufficiently well for the present purpose, which is to mention two instances of some importance where the uniformity aimed at seems to me of doubtful value—viz., the case of the proposed Royal Society Catalogue and the case of the proposed Unification of Astronomical Constants. One cannot, of course, say such things but in fear and trembling in view of the magnificence of the names backing these proposals; but sometimes a useful office is performed by the people who rush in where others fear to tread.

As regards the big catalogue, I suppose we all know that there was an International Conference last year, which settled that there was to be a comprehensive subject-index of science, and that the Royal Society was to take charge of its production. Two Committees have been formed, one to settle the plan of the index, and the other to settle the means of doing the work, and both Committees undoubtedly have their work cut out. It was at the first meeting of the latter Committee that a fact became apparent which makes one wonder whether uniformity is not being carried too far. The Committee was composed in great part of representatives from the different learned societies, and these were asked in turn to give their views as to ways and means. It then appeared that the positions and requirements of the various sciences in relation to the scheme were vastly different. In Chemistry an analyzed subject-index was a thing of necessity, but it was already in good going order. Other sciences had as yet scarcely felt the need of one. Geography had ample resources and put them at the disposal of the Committee; Mathematics had none and could do nothing for the Committee. And so on. Now the difficulty about resources can of course be got over; but the difference in the needs of the sciences is surely a real and permanent one. Is it after all likely that any scheme of classification would suit all sciences equally well? and if different schemes are adopted then the Catalogue is no longer one, but several Catalogues; and there is nothing gained by disguising the fact.

ASTRONOMY seems to me to be one of the Sciences which has scarcely yet felt the need of such an index seriously. When anyone really wishes to find out what has been done on a particular point, he can get what he wants without much trouble, and would not be much helped by the proposed index. I fancy most cases where work is done over again are due, not to the lack of means for finding out what has been done, but to neglect in using them, which is an irksome task. In his 'History of Elasticity' (vol. ii. pt. ii. p. 488), Prof. Karl Pearson writes:—"A distinguished biologist once stated to the Editor of the present work that he had for many years given up endeavouring to ascertain what others had done or were doing in his subject. To follow the great mass of contemporary work meant to expend his time in historical investigations rather than in original research. When he devoted his energies to the latter, he was fairly certain that 50 per cent. of his published results would be new contributions to scientific knowledge." I don't think it is finding the papers that is the real trouble, it is the reading them when found: and in this an index will not help us. After all, what matters it if some things *are* done twice over? Both workers get the exhilaration of doing them, which is the main thing. If we knew what was to be the end of it all, there might be good reason for hastening there as economically and quickly as possible; but as we don't, and as the journey is a pleasant one, I don't see that it is a great calamity if we get twice the usual fun out of some bits of it.

In Chemistry, as above stated, with its multitudinous compounds, there is a clear need for a certain kind of index. In an ideal index of Astronomy of the same kind should not every star be entered separately? A good many of them have little histories of their own already, and the number is daily growing. It is not inconceivable that something of this kind may ultimately become an astronomical necessity; but it is difficult to see how it could be incorporated with indexes of other sciences. I don't think we can yet form a very clear idea of what our needs will be even in the immediate future; but it seems probable that a reference-list of existing photographs will be nearly as important as a list of published papers; and this again would not readily tack on to any scheme including the other sciences.

It seems rather disloyal to say so much in opposition to a scheme so fairly launched. We owe a great deal to the existing 'Royal Society Catalogue of Scientific Papers' under author's names; and the proposed index, in any form, would no doubt be of immense value. But will it be worth the enormous labour, which might be used in other ways? and is it possible to do it? It is a herculean task, and many shake their heads in private over it, though they are reluctant so withhold their support. It is true the International Conference, composed of eminent men, was in favour of the attempt; but it is easy to vote for somebody else to do some work. I don't think those who will have to do the work, if it is to be done, were given a fair chance to have their say.

It is a common fault of Conferences to fail to be representative. The people who vote are not always the people most concerned, and hence arises trouble. Passing to the other matter mentioned in the first paragraph, the proposed Unification of Astronomical Constants, it may seem a paradox to say that the people who voted were not those chiefly concerned, but it is tolerably near the truth for all that. The people most affected by a change in the Constants of the Nautical Almanacs are not those who make the

Almanacs, but those who use them, and hence a Conference of Superintendents of Almanacs, such as was held last year in Paris, is scarcely the proper body to decide on such a change. It is easy to make this discovery now, for the simple reason that the users of the Almanacs are already up in arms against the decisions of the Conference. Professor Lewis Boss leads the attack in the 'Astronomical Journal,' No. 410, though it is, perhaps, unjust to him to use such a warlike metaphor to describe his very temperate, but still adverse, criticism of the proposals. But there is no doubt about the hostility of the able Editor of the Journal. Mr. Chandler in the same number proclaims himself openly on the side of the insurgents, and invites astronomers generally to range themselves on one side or the other, or proclaim their neutrality. In all seriousness it is to be earnestly hoped that Professor Boss's remonstrance will be regarded; and the best way of securing this is to give him all possible support. Unless the change can be made with practical unanimity, it would only do harm to try it; and it seems clear that the proposals made at Paris are open to serious objections.

THESE paragraphs are, I fear, a little heavier than the usual run, especially in holiday time. I have not been fortunate lately in coming across things of a lighter kind. A kind but anonymous correspondent sent me the other day from Dublin a sentence from the 'Daily Nation' of June 17, wherein "Mr. Dillon boasted that his resplendent star was high and culminating on the horizon," which is a tolerably complicated thing for a star to do; and I stumbled on a story the other day which suggested a new view of meteors as peculiar to certain localities. An air of probability is thus given to the killing of the villain by a meteorite, which is fresher than the usual death by lightning. I don't think there is any doubt as to the author's intentions, which are clearly expressed in the following sentences:—

> "By Jove! you go in heavily for meteors in this part of the country," exclaimed the other abruptly, as though disregarding his companion's homily.
>
> The evening was fast closing in upon them, and a brilliant shower of falling stars was the cause of Hopper's remark.
>
> "Yes, they even possess a meteorite here. The point where it struck, and the hole in the cliff which this alleged visitor from another planet made, is carefully guarded. It must be just here, I think."

But beyond these my net has caught nothing of any importance recently that bears on Astronomy. Most of the cuttings of the last month seem to relate to other subjects, such as cricket. For instance, I see one clipping contains a sentence of rather unexpected humour at the end of one of Prince Ranjitsinhji's articles, to wit:—

> A man who is engaged in heavy brain-work, such as writing a book on cricket, or trying to matriculate at a Cambridge College, cannot expect to be at his best in the cricket-field.

But I am not sure whether the Editor will think this suitable for insertion in this Magazine.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 258. OCTOBER. 1897.

On the Photography of Planetary Surfaces.

[Continued from p. 341.]

Best Size of Instrument.—The question of the best size of instrument to use for planetary work is one that has been much discussed. But it will be generally admitted, I think, that the question is not by any means definitely settled. There are two definite properties of an objective which are of fundamental importance in determining its efficiency in any planetary work either visual or photographic, and neither of these has been as generally recognized and taken account of as is desirable and necessary in a full discussion of the question. These are: (*a*) the "resolving power," and (*b*) the "delineating power," or "contrasting power." The first defines, as is well known, the power of the objective to "resolve" or show as separate objects two or more close lines or points on the surface of the planet, and the limit of resolution α (the angle between two points or fine lines that just appear separated) is represented by the well-known expression.

$$\alpha = m\frac{\lambda}{b}.$$

where *m* varies from unity for a rectangular aperture (of width *b*), to about 1.1 for a circular aperture of diameter *b*.

The second property defines the visibility or distinctness of a faint object, which depends primarily on the "contrast" between its image and that of the general field or background against which it is seen or photographed. The conditions which determine this contrast between various forms of radiating sources (points, lines, and surfaces of finite or infinite extent) have been investigated by the writer in a recent paper in the 'Astrophysical Journal' ("On the Conditions of Maximum Efficiency in Astro-photographic Work.—Part I. General Theory of various Forms of Radiating Sources," Aug. 1897, p. 119); and the results have been applied to the special case of linear markings on planetary surfaces in another paper to be published shortly. The expression there obtained for the "contrast" between the image of the general planetary surface and the image of a linear marking on that surface is

$$\left.\begin{aligned}\kappa-1&=\frac{8}{3\pi}\cdot\frac{k}{k_{\prime\prime}}\cdot h\cdot\frac{b}{\lambda},\\ &\cong\frac{k}{k_{\prime\prime}}\cdot\frac{h}{a}\end{aligned}\right\}\quad\ldots\ldots\quad(3)$$

and the limit to the delineative power, Δ (corresponding to a difference of 2 per cent. in intensity between the images of line and general surface), is

$$\frac{1}{\Delta}=\frac{k}{k_{\prime\prime}}=0{\cdot}02\,\frac{a}{h}.\quad\ldots\ldots\quad(4)$$

In the case of fine linear markings both the "resolving power" and the "delineative power" are therefore directly proportional to the linear aperture, *b*, of the objective; and were it not for the effect of atmospheric disturbances which determines the value of *h* (the factor which represents the decrease in the intensity of the image of the line due to the effect of atmospheric aberration), both the fineness of detail observable on the surface and the distinctness of the markings would increase indefinitely with the size of the telescope. But under the conditions met with in practical observation, *h* will decrease at first slowly, but afterwards much more rapidly than *b* increases. Hence, Δ (4) is a minimum (or the contrast is greatest) for a certain value of *b*, which, as shown in the paper already referred to, is not likely to exceed 30 to 35 inches under the *best* conditions of "seeing" practically attainable in *visual* observation. For photographic work the limit of efficient size is undoubtedly considerably smaller because of the difficulties already referred to in the first part of this paper—difficulties which are introduced by the time element in the photographic method, and by the nature of the summative process employed by the sensitive plate. These difficulties, although minimized by the suggested method of intermittent exposure, can never be completely got rid of except when the exposure is practically instantaneous, as in the case of the Sun. The longer the exposure time the more serious they become, and it is easy to see that they will also rapidly increase with the size of the aperture. For the aberrational effect of a given atmospheric disturbance will vary in general as the third or fourth power of the aperture, and the duration and frequency of intervals of "good seeing" may be considered to vary (at least roughly) in the inverse ratio. Since the definition in all cases is represented by a resultant of effects which must be extended over a longer period in photographic work than in visual observation (the eye would profit by a much shorter interval of good definition than could be utilized photographically, even on the intermittent exposure plan, on account of the time required to uncover and cover the plate), it will, for the reasons just stated, be more injuriously affected by an increase in aperture in the former class of work. In general it would seem safe to assume that for the larger apertures the average value of h for the whole time of photographic exposure would not be more than $\frac{1}{3}$ to $\frac{1}{2}$ as great as the average value for the moments of best definition utilized by the eye. The *maximum* efficient aperture of objective for planetary photography would not, under these conditions, exceed 10 to 15 inches.

The *minimum* linear aperture which should be used is more easily determined. We have already spoken of the necessity for obtaining a certain minimum size of image in order to avoid too great subsequent enlargement. If we assume this minimum size to be at least 0·1 inch, then the *focal length* (actual or equivalent) of the photographic objective would have to be, in the case of Mars at its nearest approach, at least 67 feet. With plates of the mean fineness of grain assumed in I. ($e = 0{\cdot}01$ mm.) the *linear aperture* of the objective should be from (2)

$$b = 10{\cdot}4 \text{ inches.}$$

Whatever may be the *maximum* limit imposed by considerations of delineative power (which are necessarily more or less variable and indeterminate), there are two other important reasons why the use of an aperture larger than about ten inches is unnecessary, or at least superfluous, in planetary photography. The first of these is that there is a certain limit to the accuracy with which the image can be "followed," or kept fixed relatively to the sensitive plate. The question how accurately is it necessary to "follow" in order to avoid injuring the photographic resolution is a pertinent and important one. If the image shifted gradually and uniformly, the integrated effect (in time) for a fine line which moves an angular distance σ would, for continuous exposure, be represented by a diffraction-pattern in which the distribution in intensity would be exactly the same as in the pattern of a stationary line of uniform intensity, and of a width equal to the angular movement σ of the first line. In my paper "On the Resolving Power of Telescopes and Spectroscopes for Lines of Finite Width" (Phil. Mag., May 1897), it is shown that the angular limit of resolution of a telescope of linear aperture b for a line of width σ is

$$\Sigma = \sigma + \frac{a^2}{2\sigma + a} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where a is the theoretical resolving power of the instrument for lines of negligible width, as already defined. The expression (5) has a minimum value for $\sigma = \frac{1}{5}a$ and is equal to a for $\sigma = \frac{1}{2}a$. Or, under the conditions supposed above, the photographic resolution would not be affected by a uniform shift amounting to not more than $\frac{1}{2}a$ during the whole time of exposure. A shift of $\frac{1}{4}a$ might theoretically even improve it.

In general, however, the movements of the image would be of an irregular character, and even if they were regular the exposure, if the above plan of working was adopted, would not be continuous, but intermittent. For this reason and for the additional one that the total actual exposure time will be relatively short (and consequently irregular movements will play a more important part in the whole exposure than they would with long exposures), we may conclude that if the movements of the image during actual exposure be allowed to amount to one-half the resolving power of the photographic objective, they certainly should not be allowed to exceed this. For a 10-inch aperture this means that the maximum displacement should be between $\frac{1}{5}''$ and $\frac{1}{4}''$ (see Table I.), a degree of accuracy in following that, in the opinion of the writer, might be just possible under the very best conditions. This is a higher degree of accuracy than some will admit it practicable to attain. My opinion is based on the very brief experience with the new Yerkes telescope, and on my experience with electric controls on the siderostat at Washington. The oscillations (rapid movements) of the star-images in the field of the former instrument did not exceed one and a half times to twice the width of the star-image, *i. e.* about $\frac{1}{3}''$, under quite unfavourable atmospheric conditions, and before the polar axis and driving-gears of the instrument had been carefully adjusted. I know from my experience with the siderostat (which, as everyone knows, is far more difficult to handle than the equatorial) that slow motions of the image, whether periodic or not, may be corrected with the utmost rapidity and facility with a properly arranged system of electric controls. It may not, however, be possible to follow with the degree of accuracy assumed above with the equatorial, although I fully believe it will be, with a properly constructed polar heliostat. *If it is not possible it means simply that even smaller apertures than ten inches will give us all the detail that we could obtain with larger ones, and renders still more evident the superfluity of using instruments of* 15, 20, *and even* 30 *inches* (as has been previously done) *for the purposes of planetary photography.* For a 30-inch aperture, for example, the accuracy of following required to render full photographic resolution possible would have to be three times as great as with a 10-inch, *i. e.* the displacements could not exceed $0''\cdot07$. This is a degree of accuracy which, at present at least, it is certainly impossible to attain. And considering the greatly increased difficulties attendant upon even slightly increasing the accuracy of following beyond the limit which was assumed above, it hardly seems probable that we will *ever* be able to utilize (photographically) the full resolving power of apertures larger than twelve to fifteen inches, except when the exposure is practically instantaneous, as in the case of the Sun.

In the case of all the planets there is another argument almost as conclusive against the use of larger apertures. The larger the aperture, the shorter and less frequent the moments of good definition, and therefore, as already pointed out, the longer the total time required, on the intermittent exposure plan, to obtain a full exposure. With an angular aperture $\frac{1}{80}$, the total exposure time required for Mars is from 2 to 5 seconds. Assuming five per cent. of the time to be suitable for exposures with a 10-inch aperture, the total time required for obtaining a full exposure negative will be, allowing for time required to cover and uncover the plate, &c., from one to two minutes. With a 40-inch aperture, if we assume as a first approximation that the percentage time of good seeing is inversely proportional to only the square of the aperture (probably, as already pointed out, it will, as a rule, be more nearly inversely as the third or fourth power of the aperture), the total time required for getting a negative will be from fifteen to thirty minutes. Now in this time the movement of the surface-markings due to the rotation of the planet itself will have far exceeded the limit imposed above. This is a point which, curiously enough, seems to have been hitherto disregarded, perhaps because it was considered unnecessary to take account of this effect until other sources of bad photographic definition had been more completely eliminated. The angular shift in seconds of arc of a point near the centre of a planet's disk per minute is evidently

$$\epsilon = \frac{21600}{M} \cdot 0{\cdot}00029 \cdot \delta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where M is the time (in minutes) of axial rotation, and δ the apparent angular semidiameter (in seconds) of the planet.

In the case of Mars, $M = 1477{\cdot}4$ and δ (at maximum) $= 12''{\cdot}5$. Therefore ϵ amounts to about $0''{\cdot}053$ per minute; hence (with a 10-inch aperture) not more than four minutes at the most can be allowed for the operation of taking the negative. In the case of Saturn under the same circumstances, the angular shift per minute (on the surface of the planet) is about twice as great, and in Jupiter about *five times* as great as on Mars. It is therefore evident that the large apertures and long exposures which have in some cases been given to these planets have been in themselves self-destructive of detail. In the case of Jupiter, for example, the longest exposure which it would be possible to give without interfering with the utilization of the full photographic resolving power of a 10-inch aperture would be about 45 seconds. Yet some have given an exposure of over 6 minutes on Saturn, and nearly 2 minutes on Jupiter, and this with a still larger aperture (13 inches). In the case of Jupiter an exposure of from 1 to 2 seconds is required with an aperture of $\frac{1}{80}$; hence in the case of this planet, as in the case of Mars, a 10-inch aperture is, with the intermittent exposure method, about the limit of possible size.

To bring out these last two points, which seem important ones, a little more clearly, I have computed the following table, which gives approximately the maximum allowable error of following and the maximum duration of exposure permissible with different apertures, on the assumption that the movement of the image must not exceed one-half the angular resolving power of the telescope.

Considerations of focal length do not enter in this case, except in so far as they determine the photographic resolution. It is assumed that the angular aperture is at least small enough to enable full photographic resolution to be attained, otherwise we might as well fall back upon a smaller aperture at once.

TABLE I.

$\lambda = 4307$ tenth-metres.

Aperture in inches.	Photographic resolving power in ″.	Max. allowable error in guiding $= \frac{1}{2}a$.	Max. allowable exposure time in seconds.		
			Mars at Perihelion.	Saturn at Perihelion.	Jupiter at Perihelion.
		″	sec.	sec.	sec.
6	0·64	about $\frac{1}{3}$	360	180	72
9	0·44	" $\frac{1}{4}$	240	120	48
12	0·32	" $\frac{1}{6}$	180	90	36
15	0·26	" $\frac{1}{8}$	144	72	29
20	0·19	" $\frac{1}{10}$	108	54	22
25	0·15	" $\frac{1}{13}$	86	43	17
30	0·13	" $\frac{1}{16}$	72	36	14
40	0·096	" $\frac{1}{20}$	54	27	11

So much for the question of the best linear and angular aperture and the best general method to use in photographing the planets. From the preceding considerations we see that the limits for efficient working are rather narrow as regards both the former questions. The demands for a certain minimum size of image (at least 0·1 inch), and at the same time as great brightness of image as is consistent with full photographic resolution, requires (for $\beta = \frac{1}{80}$) that the linear aperture be at least 9 to 10 inches. On the other hand this aperture cannot be much exceeded with efficiency, because if much larger the effect of atmospheric disturbances becomes more serious, and because with much larger apertures it is impossible to follow with sufficient accuracy to realize their theoretically greater resolving power. With our present instrumental means the limits of efficient size seem to lie between 8 inches and 12 inches. When we have realized the full resolving power of these instruments photographically, we shall be better prepared to turn our attention to the serious instrumental and atmospheric difficulties attendant upon the use of still larger instruments. But, as stated before, it is doubtful whether we shall ever succeed in utilizing photographically a resolving power greater than that of a 15-inch aperture.

Yerkes Observatory. July 1897. F. L. O. WADSWORTH.

W. F. DENNING.

From a photograph by Mr. J. Webb, Bristol.

From a photograph by Mr. J. Webb, Bristol.

The Great Meteoric Shower of November.

[Continued from p. 348.]

Observations necessary.

As it appears highly probable that we shall have to depend, for some time to come, upon the old method of eye-estimation in ascertaining the leading features of a meteor shower, it is proposed to treat of the subject from this standpoint alone. No doubt photography will be again brought into service in attempts to get some impressions of meteor-trails, and it is hoped that the experiment will not prove wholly unsuccessful.

In this class of observation practice is indispensable, and not much really good work can be accomplished until the observer has made himself familiar with its requirements. Our previous knowledge in this department has been gained by persistent effort and careful observation, which, however, have been far from absolutely accurate, and could not be otherwise from the nature of the performance. The paths of meteors, as recorded, are but hurried and often rough eye-estimates of position, consequently a large probable error must naturally attend them. These, however, can be reduced to very moderate limits by habitual training, and especially in cases where the observer possesses aptitude for this kind of work. There is (and obviously must be, owing to the great distinctions existing in individuals as to their relative capacity for a labour which requires a good eye, untiring patience, quickness of conception, and a sound judgment) a great difference as to the probable error in various cases, for one person in fixing radiants may show a mean error of 6° or 7°, while another may be well within 2°. Unfortunately there is no school for educating aspirants to proficiency and fame in this field, nor is there any tribunal where observers may come up for trial and judgment as to the accuracy of their results. But the degree of exactness attained by anyone may be pretty well gauged by comparing the materials he has gathered with the average of the best results obtained by other observers.

The observation of a brilliant shower of streak-leaving meteors, like the Leonids, is much less difficult, complicated, and perplexing than that entailed in the general investigation of minor streams. The paths of Leonids can sometimes be registered very precisely by perfect novices in this particular work, as the streaks of prominent meteors remain some seconds, if not some minutes, and their exact location amid neighbouring stars can be marked on a globe or map by anyone acquainted with the constellations. It must be done, however, in each case, as quickly as possible after the nucleus itself is spent, for the streak begins to drift immediately after the moment of its first projection, and it is only at this time that it correctly represents the path pursued by the nucleus.

As to the observations necessary to be made, they may be described as manifold. It is not sufficient to stand and merely count the numbers visible at certain intervals, and jot down a few notes of the brighter objects. The phenomenon requires critical watching with regard to certain important features, and among these the following may be instanced:—

1. The exact time of maximum abundance.
2. The number of meteors visible per minute at max.
3. The position of the centre of radiation.
4. The diameter of the area of the radiant, if diffuse.
5. The duration of the shower's chief activity.
6. The whole duration of its visibility (from about Nov. 7 to 20).
7. The relative magnitudes of the meteors.
8. The durations of their visible flights.
9. The colours of the meteors and of their streaks.
10. The durations of the streaks and extent and direction of their drift. In the case of very bright streaks enduring a long time a series of positions for the central part should be taken say at intervals of 2 or 3 minutes.
11. The apparent paths of the brighter meteors. Many of these will be sure to be seen at several stations, and if properly recorded the data will enable their real paths to be derived.
12. The places of apparition of stationary meteors.
13. The number of meteors proceeding from other showers. These will probably not amount to more than 10 or 12 per hour, and need not be considered during a really brilliant return of the Leonids. During a feeble display, however, meteors diverging from the minor systems should be carefully separated from the Leonids or the real strength of the latter will be exaggerated.
14. The shape of the nuclei and their apparent diameters in the case of bolides.
15. Instances of curved paths, broken streaks, and other visible peculiarities.

After the disruption of large meteors the observer should listen for some minutes with the idea of noticing any audible detonation should it be possible.

Of course more than two observers must be employed to thoroughly record the characteristics of a meteoric display. One should count the numbers visible during, say, every successive period of 5 minutes, and record the totals, so that the rate of increase, time of max., rate of decrease, and the duration of great activity may be severally determined. Another will register the paths of individual meteors, determine the place of the radiant at, say, half-hourly intervals, while a third will derive the relative magnitudes by watching one particular quarter of the heavens and recording the various magnitudes of all the meteors which appear there. By separating the materials collected in this way into half-hourly intervals, it might be seen whether any variation occurred during the rise, height, and fall of the display, or whether the altitude of the radiant exercised any influence upon the apparent brilliancy of the meteors.

One observer, in relying upon his own exertions, will perhaps do best by counting the meteors during every alternate five minutes, and recording paths and other details in the intervals. Should meteors be too numerous for exact enumeration, the number ought to be carefully estimated. Or by watching a very contracted region of the sky (which might be done by standing far back from an open window, or by looking through an empty telescope-tube, or by some other device which the observer might readily arrange for himself), the number of meteors appearing in the field might be counted; and the diameter and altitude of the space under view being known, as well as the average length of the paths, a computation might easily be made as to the number of meteors which appeared in the whole sky.

The greater the number of observers the larger the total of meteors that will be seen. Experiments have been occasionally made to ascertain the relative numbers: thus during the Leonid display of 1865 Prof. Newton obtained the following figures * from a simultaneous watch of different parts of the heavens by 12 observers:—

No. of observers.	No. of meteors seen.	No. of observers.	No. of meteors seen.	No. of observers.	No. of meteors seen.
1....	325.	5....	1106	9....	1399
2....	633	6....	1200	10....	1451
3....	836	7....	1282	11....	1509
4....	1000	8....	1348	12....	1560

Prof. Newton was led to assume that the total number visible in the whole firmament was 2400, so that one observer may expect to see more than one-eighth of the aggregate, while six observers will record one half.

In determining the time of maximum the altitude of the radiant should be considered, for the visible strength of a shower is in part regulated by its zenithal distance. The hourly number of meteors for a radiant at zenith may be computed by multiplying the number actually counted in one hour by the secant of the zenith-distance of the radiant. The number so deduced will represent the total number of meteors that would have appeared from the radiant had it occupied a place in the zenith.

The altitude of the Leonid radiant on Nov. 14 at various hours of the night is, approximately, as follows:—11^{h} 6°, 12^{h} 14°, 13^{h} 23°, 14^{h} 33°, 15^{h} 41½°, 16^{h} 49°, 17^{h} 56°, 18^{h} 61°. This is for

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* Dr. Kleiber gave a similar table in Ast. Nach. 2621.

the latitude of Greenwich, Bristol, and other places in nearly the same parallel.

To obtain a really good radiant for the shower it will be necessary to determine this point say for every quarter or half an hour, from a few good paths. Then at the end of a few hours a number of independent positions will be available from which to derive a mean centre. This is a much better method than getting one radiant from a large number of paths.

As to the minor showers of the period they may well be disregarded, as far as the determination of their radiants is concerned, during a rich return of the Leonids. The former may be more suitably studied in years when the great periodical shower is in feeble evidence and when nearly the whole of the observer's attention can be directed to them.

The path-direction of a meteor may be retained for a few seconds, by holding a perfectly straight wand at arm's length and projecting it upon the sky consistently with the course of the nucleus or position of the lingering streak, and carefully noting the stars in the backward and forward extension of the line of flight. To rely upon the eye alone is not so satisfactory as it is liable to induce untrustworthy impressions, so that it is advisable to employ the wand as an assistant and a corrective.

When determining the radiant the observer has to concentrate all his efforts on getting the direction of flight correctly, and the exact length of path is of little consequence. But in cases where observations are specially made for the purpose of determining the real paths in our atmosphere it is necessary to record both the direction and beginning and end points of flights with the utmost accuracy.

As to the actual duration of the individual Leonids, it must be admitted that no really satisfactory method of obtaining it seems applicable. The majority of the meteors have a visible existence which is comprised within one second of time, and in previons years the rough estimates which have been made of these transient intervals of visibility have shown little accordance. An error of one fifth of a second in such estimates means an error of about 10 miles in the velocity per second of one of these meteors, and thus this element appears to have been very doubtfully assigned. On 1896 Nov. 14 five Leonids were seen with computed velocities of 33, 45, 46, 58, and 63 miles respectively, but errors of observation probably induced the differences between the several values and the theoretical speed of 44 miles per second.

It would be well if observers adopted one uniform method of registering the paths of meteors observed. This would greatly facilitate the reduction and comparison of such materials. The plan of registry adopted by Lieut.-Col. Tupman and some others is both simple and effective, and to illustrate it I will give a table of fine Leonids seen at Bristol at the returns of 1879 and 1888 :—

Date.	G.M.T.		Mag.	Observed Path.				Path.	Notes.
				From		To			
	h	m		R.A. °	Dec. °	R.A. °	Dec. °	°	
1879									
Nov. 13 ...	11	9	1	90	+12	48½	− 4	43	Sw., stk.
13 ...	15	6	♃	98	+37	77	+36	17	V. sw., B. stk.
13 ...	15	13	♂	153	+34	157	+39	5½	V. sw., B. stk.
13 ...	15	28	1	119	+14	107	+ 9½	13	V. sw., B. stk.
13 ...	16	16	♂	187	+42	205	+44½	13	V. sw., stk.
13 ...	16	41	♃	184	+30	193	+30	7½	V. sw., stk.
1888									
Nov. 13 ...	16	41	♃	166	+34	172	+37½	6	V. sw., B. stk.
13 ...	16	55	½=☽	125½	−22	121	−30½	9½	Sw., B. stk.
13 ...	17	19	♃	252	+33½	257½	+31	5½	Sw., stk.
13 ...	17	46	1	175	+66½	210	+75½	14	Sw., B. stk.

The "durations of flight" were not estimated, but a column to express these should be always included in such tables. The abbreviations are:—"V.," very; "sw.," swift; "B.," bright; "stk.," streak.

Observers intending specially to watch the region of the radiant-point and re-determine it must necessarily take up a position commanding a good open view of the E.N.E. sky near the horizon before midnight. The radiant rises at a point about 40° N. of East, and is due east at about $13^h\ 45^m$ on November 14 at an altitude of 31°. If the primary aim is to secure double or multiple observations of bright meteors then the observers, located of course in different parts of the country, might disregard the radiant and direct their watch towards a point over the Midlands. Were this plan followed it is likely that such prominent meteors as appeared over the central parts of England would each have several observers, and might admit of the very satisfactory investigation of their real paths in the air.

In concluding our reference to that magnificent meteoric display which, though it is now an object of admiration to us, proved a source of terror to our forefathers, it may be added that though some disappointments will be sure to attend its attempted observation in the few ensuing years, it cannot wholly escape the vigilant observer. Clouds may obscure it in one year, but a clear sky will unveil it in the next. It is fortunate that the Earth passes through the stream of meteors in several years, and thus allows more than one opportunity for observation. Fine weather often prevails at the middle of November. That the shower will be thoroughly well observed cannot be doubted, and that it will enable some new facts to be elucidated is highly probable. The showers of 1833 and 1866 each gave a remarkable impetus to the study of meteoric astronomy; may those of 1899 and 1900 fulfil a similar purpose! At the epoch of the last return the researches of Schiaparelli, Newton, and Adams practically revolutionized our knowledge of meteors, and placed its theories on a well-assured basis. The phenomena of coming years will greatly encourage its renewed progress, if they do not lead to triumphs equal to those of 1866, and attract earnest attention to a most engrossing branch of science.

The November Shower from Beila's Comet.

Apart from the Leonids, which undoubtedly form *the* "great meteoric shower of November," there is another system of considerable strength and many interesting associations, which makes its periodical apparitions in the month of November. It has a period of about 6½ years and presents very close orbital resemblance to Biela's Comet. It furnished a shower of about 2000 meteors on 1798 December 6, and recurred brilliantly on 1838 December 6 and 7. In 1872 and 1885, on November 27 in each year, very abundant showers were observed, and also in 1892 November 23, when at Princeton, U.S.A., more than 20 meteors per minute were counted at 10 and 11 P.M. The difference of 4 days between the showers of 1885 and 1892 was brought about, according to Bredichin (Ast. Nach. 1219), by perturbation due to Jupiter, which caused a recession of the node to the extent of a little over 4°.

The radiant-point of this meteor-shower is in Andromeda, at 25° $+43°$, near the star γ. The meteors are totally different to the Leonids, for the Andromedes move very slowly (having virtually to overtake the Earth in her orbit) and leave trains of yellowish sparks. The radiant is visible all night, being circumpolar in our latitude.

A brilliant return of these meteors ought to be witnessed in 1898 on Nov. 23, as a similar period will have lapsed to that which separated the splendid displays of 1872 and 1885. But the Moon will be full on the morning of November 28, and on the important night (Nov. 23) will be 11 days old and visible at Greenwich throughout the evening and until $3^h\,24^m$ on the following morning. The exact time of reappearance of the shower cannot be definitely foretold, but inasmuch as, in 1892, it formed a brilliant display for 5 hours before clouds obscured it, it will scarcely be able to elude general observation should it reappear in 1898. Observations should be made after moonset ($2^h\,10^m$ A.M.) on the morning of November 23 and again at intervals during the night following that date. The ordinary observer need not maintain a persistent watch of the firmament in order to discover the evidences of it; an occasional look-out at intervals of an hour or two would be sufficient to detect it directly it assumed an imposing stage.

Thus in 1898 November observers are offered a double prospect. They may see a fine exhibition of swift streak-leaving Leonids on the morning of November 15 and an equally abundant display of slow, train-bearing Andromedes on November 23. The latter shower may even exceed the former in point of numbers, for the showers of Andromedes in 1872 and 1885, as meteoric spectacles, have never perhaps been excelled except by the magnificent display of Leonids which occurred in 1799 and 1833.

W. F. Denning.

Standard Star Places.

In Nos. 391, 398, and 408 of the 'Astronomical Journal' will be found a discussion of the results of the observations of some 310 ephemeris stars made at Lick during the years 1894–6 by Mr. Tucker, a leading feature of which is the general excellence of the observations, which were made with a Repsold 6-inch reversible transit-circle. The only description of this instrument to be found in vol. i. of the 'Lick Observatory Publications' is a certificate from Professors Auwers and Krueger that they had examined and tested it and found it to be in every way suited to be the chief instrument in an observatory of the first class, and certainly the results appear to justify the statement. The probable errors for the mean of eight observations vary in R.A. from $\pm 0^{s}\cdot010$ sec δ up to 70° declination to $\pm 0^{s}\cdot004$ sec δ for circumpolars observed at both culminations, and in declination from $\pm 0''\cdot25$ at the extremes ($-40°$ declination and $+70°$ L.C.) to $\pm 0''\cdot11$ at the equator. Some years ago, at the request of Dr. Gill, many observatories joined in a series of observations of stars to be used in connection with heliometer observations of the small planets Iris, Victoria, and Sappho, for the determination of the value of the solar parallax. These observations have been discussed by Prof. Auwers and lately published in vol. vii. of the 'Annals of the Cape Observatory,' and one of the most striking features of the discussion is the remarkable excellence of the modern reversible transit-circles. The probable errors of an observation in Right Ascension and Declination with one of these instruments range from $\pm 0^{s}\cdot02$ to $\pm 0^{s}\cdot04$ and from $\pm 0''\cdot3$ to $\pm 0''\cdot5$, quantities which are just about half the amounts given by the results with the older non-reversible instruments. Perhaps a study of the absolute weights apportioned to the results of the different observatories on pp. 702 and 703 will be as good an object-lesson as anything in this particular subject.

In the course of the observations Mr. Tucker noticed that his observations in Right Ascension had a systematic difference of $-0^{s}\cdot029$ taken in the sense S.–N., depending on whether the observations were made facing north or south of the zenith. Such a discordance may be personal error due to the direction of the star's motion, but it is quite as conceivable that in the case of eye-and-ear observing with a clock the place from whence the sound originates may be as fruitful a source of personality as the apparent direction of the motion of a star with regard to the wires. We all know cases of deaf people who have got their good and bad ear. In the case of galvanic registration the method by which contact is made becomes equally important, because, in the case of a fixed button on one side of the instrument only, which for many years was the case at Greenwich, the observer in changing from one side to the other of the zenith has to use different hands in order to register the time.

The following comparison by Mr. Tucker of the relative positions of the fundamental stars, as given in the 'Berliner Jahrbuch,' 'American Ephemeris,' 'Connaissance des Temps,' and the British 'Nautical Almanac' for 1895, shows the want of uniformity among the different ephemerides:—

	s	″	
B. J.—A. E.....	−0·027	+0·01	For 51 stars.
B. J.—C. T.....	+0·015	−0·24	53 "
B. J.—N. A. ..	+0·022	+0·19	56 "

The Right Ascensions of the 'American Ephemeris' are founded on the well-known system of Right Ascension given by Prof. Newcomb in his 1850 Catalogue, those of the 'Berliner Jahrbuch' on that given by Prof. Auwers in his Catalogue for 1875 published in vol. xiv. of the 'Astronomische Gesellschaft,' those of the 'Nautical Almanac' are derived from the Greenwich and Cape Catalogues for 1880, with the use of Auwers's proper motion, and those of the 'Connaissance des Temps' from an unpublished source. By the help of recent papers on this subject it will be easy, and may be interesting, to portion out these discordances. For this purpose we must have a standard, and for it we propose to take the Greenwich Sun observations 1836–1895, as given in a paper communicated to the 'Monthly Notices,' vol. lvi. 9, pp. 466–471. From the materials there given the correction to the different Greenwich Catalogues to reduce them to this system of Sun observations is as follows:—

Catalogue.	Correction.	Catalogue.	Correction.
	s		s
1840......	+0·036	1872	+0·09
1845......	−0·013	1880	+0·17
1850......	−0·022	1890 (5 year)	+0·28
1864......	−0·004		

Now let us assume that any system of Right Ascensions requires a correction of the form $x+y$ T, where x is the correction to the Catalogue, y is the correction to the adopted proper motions and precession constant or the centennial variation, and T is the fraction of a century. Then the values of x for this purpose in the case of Newcomb's system are derived from a direct comparison between the Greenwich 1850 Catalogue corrected as above, and that of Newcomb's for the same date, and in the case of Auwers's system from a direct comparison between the Greenwich

1872 Catalogue corrected as above and that of Auwers's for 1875, using Greenwich clock-stars only. The value of y for reducing Auwers's adopted system to the chosen standard is given in the above paper as $+0^{\mathrm{s}}{\cdot}132$, while in A. J., No. 359, Prof. Newcomb has given the value of y as $+0^{\mathrm{s}}{\cdot}120$ to reduce Auwers's system to his own system. Therefore the difference of these two quantities, viz. $+0^{\mathrm{s}}{\cdot}012$, may be taken as the value of y to reduce Newcomb's system to that of the Greenwich Sun observations 1836–1895, a quantity which agrees very closely with the result obtained by a comparison of the proper motions of 98 clock-stars derived from Greenwich observations only (referred to the Greenwich Sun system, 1836–1895) with those used by Prof. Newcomb in his standard stars. Thus the Greenwich Sun system 1836–1895 gives the following corrections:—

To Newcomb's system	$-0^{\mathrm{s}}{\cdot}018$	$+0^{\mathrm{s}}{\cdot}012$ T
To Auwers's system	$-0{\cdot}012$	$+0{\cdot}132$ T

where T dates from 1875; and the corrections to the ephemerides for 1895 work out as follows:—

B. J.......	$+0^{\mathrm{s}}{\cdot}014$	C. T.......	$+0^{\mathrm{s}}{\cdot}029$
A. E.	$-0{\cdot}016$	N. A.	$+0{\cdot}037$

(The value for C. T. is obtained by adopting Mr. Tucker's value of B. J. − C. T. $+0^{\mathrm{s}}{\cdot}015$.) Changing the signs of these corrections it will be noticed that the figures of Mr. Tucker's comparison are nearly exactly reproduced.

From this comparison it would seem that Newcomb's standard star places are growing more and more accurate as time goes on, or at any rate that they are not likely to be much in error for years to come; Auwers's Right Ascensions, on the other hand, are rapidly running off, but it is perfectly easy to correct them. As Bacon says, it is far easier to deduce truth from order than from chaos; and I doubt very much whether a new value of precession, and new proper motions of the fundamental stars, would give more accurate results a hundred years hence than the Struve-Peters constant of precession in connection with proper motions of the fundamental stars derived from taking the mean of those adopted by Newcomb and Auwers, with a constant correction of, say, $+0^{\mathrm{s}}{\cdot}0010$ to the latter. As the new value of the constant of precession lately obtained by Prof. Newcomb seems to suggest that the value of the Struve-Peters constant is well within the limits of error to be derived from present materials, and as the catalogues which are now being made, and those which will be made 50 years hence, will be the material upon which a more exact value will be formed in the future, it seems premature, as Prof. Boss asserts in A. J. 410, to make any change at present in this constant.

W. G. THACKERAY.

Some Reminiscences.—No. II.

ANY reminiscences of mine must in a great measure, I fear, centre about the Royal Observatory, where I passed fifty-one of the fifty-two years of my official life, the remaining year, from March 1852 to May 1853, having been spent in charge of the University Observatory, Durham. I joined the Greenwich Observatory in the year 1841, first as computer, although from the circumstance that my father was one of the Assistants appointed in the year 1825, in the time of Pond, I had previously heard much of the inner life of the place. I was attached to a staff of fourteen Computers organized (altogether distinct from the ordinary staff) for the reduction of the Greenwich observations of the Moon from 1750 to 1830, some 8000 in number, and their comparison with tables. Sir Charles Todd, Government Astronomer, South Australia, who carried the telegraph-line across Australia, was one of this band, and my old colleague, Mr. Dunkin (then Assistant in the newly formed Magnetical and Meteorological Department), had previously been engaged on a similar reduction of the Greenwich Planetary observations, the results of which shortly afterwards appeared in a thick quarto volume. The results of the Lunar reductions were published a few years later in two quarto volumes, the proof-sheets of which nearly all passed through my hands. I took also a considerable part in the calculation of the longitude of Valentia in Ireland, as determined in the year 1844 by transmission of chronometers, the expedition including also determinations for Liverpool and Kingstown. This was one of the latest occasions, I believe, on which chronometers were employed on such a scale for a fundamental determination, if there existed telegraphic communication. Thirty mean solar chronometers were employed, and, as regards Greenwich—Kingstown, and Kingstown—Valentia, some twenty journeys were made, comparison with the sidereal clocks of the terminal stations being made by coincidence of beats. The observing work was considerable, and the calculations were voluminous. The resulting longitude of Valentia came out $41^{m}\ 23^{s}{\cdot}23$. It happens that, in later years, two telegraphic determinations were at different times made—one in 1862, giving $41^{m}\ 23^{s}{\cdot}37$, and another in 1866, giving $41^{m}\ 23^{s}{\cdot}19$, all referring to the same point, Feagh Main. Here are three values which, though deduced from observations by different people using different instruments and methods (for the telegraphic determinations were not similarly made; I myself took part in that of 1862), differ in the extreme by only $0^{s}{\cdot}18$. What I would wish to point out is that although we should all, I think, agree in considering the telegraphic method as superior to the chronometric, we find that, in this instance, the chronometric value compares well with the telegraphic, being intermediate between the two, which practically corroborates the chronometric value.

The history of the longitude problem, both as regards navigation and astronomy, is interesting. Two hundred years ago the sailor had to depend on dead reckoning. Consider the difficulties experienced by Commodore Anson as late as the middle of the last century, when in rounding Cape Horn he unexpectedly made the land, supposing himself to be some 300 miles to the westward; and afterwards, when he wanted to reach the island of Juan Fernandez to recruit his sickly crew, having got into the latitude of the island he considered himself to be west of it when he was to the east, and so lost twelve days, during which a number of his men died. Greenwich Observatory was founded to improve the methods for finding longitude in the interests of navigation, but sufficiently good tables of the Moon had not been for very many years provided before the chronometer began to come to the front, and now the lunar method has fallen practically into disuse. Had the chronometer been earlier perfected, probably Greenwich Observatory might not at the time have been established, for when erected little further help did Flamsteed receive from the Government, his memorable work having been achieved under conditions difficult and depressing. Astronomers also had in the first instance to depend for longitude on methods purely astronomical. The transmission of chronometers, when the distance to be traversed was not too great, was felt, although laborious, to be a great improvement. But for all places within reach of the electric telegraph, the telegraphic method has given the death-blow to both, as regards fundamental work. Were the Chandler variation of latitude greater than appears to be the case, here might have been another troubling element. Longitude operations have probably reached a point of accuracy beyond which, for a time at least, it may be difficult to advance, for the exchange of telegraphic signals (that is, the determination of the difference being the clocks of two stations) is probably now less uncertain than the finding of local time, with its uncertainties of personal equation and local attraction.

Visitors to the Royal Observatory will hardly fail to remember the chronometer-room on the middle floor of the south-east equatorial building, that containing the 28-inch refractor. Here are kept the Navy chronometers, and here, too, is preserved that celebrated Harrison time-piece, for the perfection of which he received in the last century the government reward of £20,000, not entirely, however, until it was shown that others could make a similar instrument, a copy of which, constructed by Larcum Kendall, is also here*. It may not be generally known that the store of Navy chronometers was, as I have understood, originally lodged at the Admiralty. They were brought to the Observatory about the year 1821, being then few in number; and in the year 1822 public trials were instituted, money prizes of considerable value being given for the best chronometers. These trials have since been continued, and appear to have exercised considerable influence in stimulating improvement; but the giving of special prizes was, after a few years, discontinued. All government chronometers, after service on board the ships of H.M. Navy, come to the Observatory for examination of their condition, they then go for repair, after return from which they undergo trial, including trial in heat, to test their fitness for tropical climates. Those that go through the ordeal successfully are passed as available for service, those that fail going back for further examination or adjustment. The best chronometers are purchased from the public trials for the ever-increasing needs of the Navy. In the early part of Sir George Airy's time the number of chronometers on hand would reach about 100, sometimes less, though on one occasion, in 1842, there were at one time 170. The average number on hand from 1848 to 1852=103, from 1868 to 1872=190; but in late years the number has exceeded 300, and in the last year exceeded 400. The daily winding, rating, management of repairs, and correspondence connected with such a number of chronometers occupies a considerable portion of the time of the staff. The attention given to chronometers is one instance of distinctly utilitarian work done at the Observatory. In olden times two chronometers only were issued to each ship (unless a surveying-vessel, or for special reasons), the commanding officer having to provide a third, since discordance between two left a doubt as to which was in error. But this stipulation was withdrawn before I ceased to have charge of the chronometers, more than twenty years ago, three chronometers being afterwards supplied to each ship. Chronometers would at times be received from vessels that had suffered shipwreck, and having been under water were in a ruinous condition, beyond repair. I remember once issuing chronometers to an officer who shortly afterwards brought them back to the Observatory, saying that, in embarking, the chronometers had been accidentally capsized into the river. We opened the chronometers, took each one out of its brass box, but found that in neither case had the water got to the movement, and there really appeared to be little the matter with them. We placed them a little way in front of a good fire, to dry out any possible damp, and returned them to the chronometer-room, and, so far as I remember, they seemed to have sustained little injury, as judged by their subsequent performance, although we should not have cared to indulge chronometers too frequently with similar refreshing baths.

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* The date engraved on the interior plate of the Harrison time-piece is 1759, and on that of the Kendall time-piece is 1769.

As illustrating the continuous increase in the number of chronometers in store (very many more being afloat), it may be mentioned that, previous to the year 1851, they were all contained in the small room adjoining the principal library, now used as library No. 2, being removed in that year to a much larger room, one above the old computing-room (as it existed previous to the year 1888). In 1868, however, to obtain yet more space, the chronometers were again shifted to the middle floor of the south-east dome, their present position as before mentioned; but this room has now in turn become inadequate for the large number of chronometers of late years on hand.

Chronometers and the Navy lead us to think of time-signals. The first public time-signal (that is, a signal giving accurate time) was, so far as I know, that of the time-ball erected in the year 1833, over the eastern turret of the old Flamsteed building of the Observatory, the setting up of which I can just remember. At that time, and for some years afterwards, the western turret was used as a camera obscura, the pictures in which on one occasion as a young boy I recollect to have much enjoyed, the views from the Observatory being admirable, but this was eventually dismantled in order to mount therein the still existing Osler anemometer. The time-ball was said to have been proposed by Capt. Robert Wauchope, R.N., some years previously, and consists, as is known, of a black ball five feet in diameter, which is drawn to the top of a mast and discharged at 1^h precisely. It may be of interest to give here a copy of the original printed notice regarding the establishment of this signal, which was as follows:—

> ADMIRALTY.
>
> *28th October*, 1833.
>
> The Lords Commissioners of the Admiralty hereby give notice, that a Ball will henceforward be dropped, every day, from the top of a pole on the Eastern Turret of the Royal Observatory at Greenwich, at the moment of One o'clock P.M. mean solar time. By observing the first instant of its downward movement, all vessels in the adjacent Reaches of the river, as well as in most of the Docks, will thereby have an opportunity of regulating and rating their Chronometers.
>
> The Ball will be hoisted half-way up the pole, at Five minutes before One o'clock, as a preparatory signal, and close up, at Two minutes before One.
>
> By Command of their Lordships,
>
> JOHN BARROW.

A description of the ball machinery was given in the 'Nautical Magazine' for the year 1835, the illustration to which appears to have been taken from a complete drawing of the ball machinery made by my father on a scale of half an inch to the foot, which I afterwards lodged in the Observatory archives. Until the year 1853, the ball was dropped by hand, by release of a trigger, the error of a mean time clock placed near the trigger having been previously determined to the nearest tenth of a second, and written on a slate hanging to the clock, which was always kept some seconds fast, there being less liability to error in releasing the trigger at a number of seconds after the hour by the clock, than in having to deduct the same number of seconds backwards from 60 seconds. The fall of the ball was observed by another assistant in a different part of the building, for check. Since 1853 the trigger has been electrically discharged, by automatic action of a clock previously adjusted (without in any way touching the pendulum) to exact Greenwich mean time. On 1855 December 6, just before 1^{h}, when the ball was in process of being raised, the ball and mast were blown into the front court of the Observatory, in a sudden gust of wind. I saw the fall. For once no indication of Greenwich time. The ball and mast were re-instated in a way calculated to avoid similar accidents in future. In 1895 the ball and machinery underwent other extensive repairs. Since the erection of the Greenwich ball many other public time-balls and time-signals have been set up in different parts of the world. One at Deal is dropped daily at 1^{h} by direct electric action from Greenwich. The further development of time-signals is, however, much too large a subject on which to enter now.

I have been asked why the Greenwich ball is dropped at 1^{h} and why not at noon. I have understood the explanation to be that in 1833 the observatory staff was small, and two of its members having to be in readiness each day at noon to observe the Sun with the two meridian instruments it was considered better to select the following hour. There is a further circumstance, whether influencing the decision or not I cannot say. It is that, on some occasions in continued bad weather, when few stars have been observed, it may happen that a glimpse of the Sun at noon may be obtained, giving opportunity for further rectification of the clock before dropping the ball.

In my previous paper I made reference to the meridian instruments in use at Greenwich until the end of the year 1850, Troughton's transit and his mural circle. But I can carry my recollection back to the time of Pond, and well remember having seen the 25-foot Ramage reflecting telescope, which occupied a great part of the front court of the Observatory. A description of this instrument, with illustrative drawing, is given in Vol. II. of the 'Memoirs of the Royal Astronomical Society.' The date of the account is 1825, and on the drawing it is stated that the instrument "is erected at the Royal Observatory, Greenwich." It was still at the Observatory when Sir George Airy became Astronomer Royal in 1835, and is described in his recently published autobiography, p. 124, as "a large useless reflecting telescope." It was before long removed, but how, or what became of it, I never knew. I seem also to remember having been present in the transit-room in Pond's time when, in observing transits, it was the practise to write down the observed time at successive wires on a slate, and, unless some object necessary to be observed intervened, at once copy the transit into a book that laid open upon a desk at the east side of the room, entering the times for the separate wires in successive columns of one horizontal line, the mean of wires being added in another column. The entries in this book, or a transcript thereof with certain additions, appear to have served as copy for the printer, the published observations being in much the same form, for in those times little was done in the way of reduction of observations, although in the later years of Pond's time more attention than before was given thereto.

I have no recollection of the practise at the mural circle, but it was probably similar. Airy introduced the system of recording observations in indelible memorandum books which are carefully preserved. So long as two instruments continued to be employed for transits and zenith distances, that is until the introduction of the transit-circle in 1851, it would sometimes happen, in the observation of faint stars or small planets, whose places were not very precisely known, that by mischance the two observers would not fix upon the same object, thereby producing a concluded position that did not exist in the heavens. Or, again, a conversation such as the following might arise:—"You see two stars together, with a small one following," would say the transit-observer. "Yes," might reply the mural-circle man. "Observe the small one, it is nearest the place," would answer the transit-man. When it is remembered that this had to be settled before the object reached the first transit-wire, and that there being no chronograph, the transit-observer had also to pick up the second from the clock, it will scarcely be matter of wonder that mistakes were sometimes made. By use of the transit-circle these inconveniences became obviated, besides which it possessed an apparatus enabling the wires alone to be illuminated, the field remaining dark, a useful provision, considering the increasing number of small planets. And further, one observer could alone manage the meridian work.

One change of principle introduced by Airy at Greenwich (following his previous practise at Cambridge) was that, instead of endeavouring to keep the transit instrument adjusted to the meridian by correcting its position from time to time, the errors of the instrument were determined, and the necessary numerical correction made to the individual observations. Another circumstance concerning manipulation may be mentioned. In making observations of stars by reflection with the old mural circle, the mercury-trough had to be adjusted to position entirely by the observer. Its shape was longitudinal in the direction of the meridian, and, when the mercury was slightly disturbed, a string or sort of line of images of bead-like form would appear in the telescope—a principal central image with others on either side. If by chance the mercury-trough was placed a little askew, the string of images would appear inclined to the horizontal wire of the instrument, forming at the crossing-point two acute angles, and it was, I believe, a tradition in the Observatory in Pond's time that, under such circumstances, as good, if not a better, bisection of the reflected image of the star could be made.

My father, who took great interest in the progress of mechanical science, used to say that he was born fifty years too soon. He might still have said the same. His was the period of the earlier growth of the steam-engine, which itself has become again transformed. But in astronomy also, what "fresh woods and pastures new." The marvellous way in which, by the development of spectroscopic and photographic methods, our power of inquiry into the constitution and motions of the heavenly bodies has been extended from the limits of the solar system to the more distant celestial sphere has opened out a wonderful field of research, and given astronomers a new world to conquer. Intricate and costly apparatus is here no doubt necessary, beyond the reach of many; but happily plenty of other good work remains to be done, and numerous instances will occur to the astronomer of admirable results having been obtained, both in former times and in our own day, with but slender instrumental equipment. W. ELLIS.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Personal Equation: Chronographic and Eye-and-Ear.

GENTLEMEN,—

The comments from the "Oxford Note-Book" in the August number of the 'Observatory' on my paper upon Personal Equation in the 'Monthly Notices' for May seem to invite a statement on my part. My attention was called to the subject by reading a paper by Professor Newcomb in No. 369 of the 'Astronomical Journal,' entitled "On the Variation of Personal Equation with the Magnitude of the Stars observed." In that paper Professor Newcomb says: "There will, I conceive, be no two opinions as to the immediate cause of the error." It did not require a long search to find a second opinion (different) from Professor Newcomb's in the writings of the experimental psychologists. These investigators, who are men of scientific note in their respective countries, trace their intellectual lineage back to Bessel and other astronomers, and now claim that astronomers will do well to study their methods and results. To see whether their claims are well founded was the object of a long and rather intricate study, some of the results of which are given in my paper in the 'Monthly Notices' mentioned above. I believe I am the first astronomer to test this question by actual numerical calculation, and to show that the average anticipation of the chronograph by the Greenwich observers noting transits by the eye-and-ear method is a quantity of the same magnitude (as nearly as the subject allows) with the actual time of "simple reaction" to a sense impression, and that the views of Professor Wundt and other psychologists as to the essentially psychical nature of the astronomer's personal equations are pretty thoroughly confirmed.

I conceive that astronomers in general have failed to grasp with sufficient clearness the great difference between the psychical processes of observation by the eye-and-ear and the chronographic method chiefly because so few observers have practised both methods continuously for a number of years side by side. The conclusion to which I have come is that Wundt's contention is a true one, that the solution of Bessel's riddle, as he designates it, is to be found in psychological research, and that this will be sufficiently evident to any astronomers who will read the volumes of the philosophical journal 'Mind.' The first volume contains an article by the late Mark Pattison on "Philosophy at Oxford," another by Professor James Sully on "Physiological Psychology in Germany": while the volume for 1895 contains a lively discussion by Professor Kitchener, of Cornell University in Ithaca, New York State, and formerly Scholar of Brasenose College, of the mooted question whether sensorial reaction takes longer, as a rule, than "motor" or "shortened" reaction, a question on which he agrees with Wundt and Lange, and differs from Professor Cattell, a pupil of Wundt's, who does not agree with his former instructor.

The subject of Professor Newcomb's paper is one which has become very perplexing to the minds of astronomers through the fact that the distinction between the psychic processes of chronographic and eye-and-ear observations has been so frequently overlooked by the calculators.

The magazine 'Philosophische Studien,' edited by Professor Wundt, contains a number of independent researches of thoroughly scientific character which offer much astronomical interest. One of them, containing experiments by Berger and Cattell, enables it to be shown that the average coefficient of the "Helligkeitsgleichung" for chronographic observations $0^{s}\cdot013$ per unit of magnitude, as determined by Professor Auwers, is quite nearly reproduced in the experiments of both investigators. They studied the times of "simple reaction" to lights of varying intensity.

I do not here cite Professor Cattell's memoir "On Reaction Times and the Velocity of the Nervous Impulse," because the memoir involves physiological considerations and does not give the materials for the numerical comparison with the results of the astronomers. The memoir named is very interesting and valuable on many accounts, and confirms the lengthening of the time of reaction to faint impressions in the senses. From this it will be seen that the psychologists can not only teach the practical astronomers something, but quite probably learn something from them in return.

The reminiscences of the veteran W. Ellis in the same number of the 'Observatory' have added a very valuable element to the discussion which I desire to promote, in the account which he gives of his own psychic processes in chronographic observation.

I remain, Gentlemen,
Yours faithfully,
T. H. SAFFORD.

Williams College,
Williamstown, Mass., U.S.A.,
1897, August 30th.

[Prof. Safford's paper in the 'Monthly Notices,' it will be remembered, treated of the difference of a transit-observer's personal equation when observing respectively by eye-and-ear and by the chronographic method, and he collected therein examples of such differences, some of which appeared to be large quantities. It may be worth pointing out, though doubtless Prof. Safford has remarked the fact, that if an observer is *always late*, that is if his senses of touch and hearing both act sluggishly, his personal equation, from this cause, by the two methods will have opposite signs; for if he hears the clock *late* he records his transit *early*, whereas if he makes contact with the button *late* he records his transit *late*. Hence this sluggishness of nerve action has a double effect on the difference $e' - e$, as Prof. Safford's notation has it.

It has always seemed to us that personal equation is a question of *lateness* in different degree with different observers. This idea is borne out by the fact, which Prof. Safford points out, that the majority of Greenwich observers have been "anticipatory" in their eye-and-ear transits: that is, they record them early in comparison with the chronographic.—Eds.]

Miss Herschel's Comets.

Gentlemen,—

As it will next January be exactly fifty years since the death of Caroline Herschel, a few words respecting the comets discovered by that lady, to whom sidereal and nebular astronomy owe so much, may perhaps just now be of interest. The first of these was detected on the 1st of August, 1786, at a time when she was taking an opportunity afforded by her brother's absence in Germany to search for comets (she says, "in the neighbourhood of the Sun," meaning in the quarter of the heavens above where the Sun had set) and was rewarded by finding the one in question (the second of the year; the first was discovered by Méchain on January 17, and is now known to have been an appearance of the periodical comet which acquired the name of Encke's on the occasion of its return in 1818), which had, as Méchain afterwards calculated, passed its perihelion about three weeks before, on the morning of July 8. Sir William Herschel observed it first on August 19, when he found it near and not much unlike the nebula M. 3, though considerably brighter. It seemed also to have a very imperfect and confused kind of gathered light about the middle, which could hardly deserve the name of a nucleus.

The second comet discovered by Miss Herschel was found on the 21st of December, 1788, about a month (as calculated by Méchain) after its perihelion passage, and the second comet of the year, both of them being parabolic or at any rate showing no deviation from that form of orbit whilst under observation.

In 1790 Miss Herschel discovered two comets—I. of that year on January 7, eight days before its perihelion passage, and III. on April 18; the latter did not arrive at perihelion until May 21, and afterwards became visible for a short time to the naked eye.

Neither of these comets manifested any ellipticity in their orbits; of the first but few observations were obtained.

Her next cometary discovery was made on December 15, 1791; it was the only one seen in that year, and reckons as 1792 I., because the perihelion passage occurred then, on January 13. It was observed until the end of that month, and its orbit (parabolic) determined by Méchain.

Miss Herschel independently discovered Comet I. 1793 early in October, but was anticipated by Messier, who detected it on September 27. She was, however, the first to detect what was afterwards known as Encke's comet at its return in 1795, noticing it on November 7. One comet only was seen in 1797; it was discovered by three persons independently (Bouvard at Paris, Miss Herschel at Slough, and Stephen Lee at Hackney) on August 14, but Bouvard seems to have had the priority. The orbit of this was determined by Olbers and found not to differ appreciably from a parabola. This was the last of Caroline Herschel's cometary discoveries, so that she had the priority in five,—on 1786 August 1, 1788 December 21, 1790 January 7 and April 18, and 1795 November 7 (Encke's). It has often been remarked that the name Herschel reckons amongst comet-discoverers only through her, as neither Sir William nor Sir John ever discovered one of these bodies. After her, the next lady to discover a comet was Miss Maria Mitchell, who was the first to notice Comet VI. 1847, on the 1st of October in that year, little more than three months before the death of Caroline Herschel.

Yours faithfully,

Blackheath, 1897, Aug. 13. W. T. Lynn.

The Spectra of Binary Stars.

Gentlemen,—

I endeavoured some time ago to ascertain from the spectra and orbits of Binary Stars the relative brilliancy of those with different kinds of spectra. But many of the orbits which I then considered have since been rejected as unsatisfactory by the most competent authorities. Dr. See has, however, published a list of 40 orbits which he regards as satisfactory, and with very few exceptions his figures will, I think, convince most astronomers. Mr. Burnham states that he regards 36 binary stars as having satisfactorily computed orbits. He does not give a list, but the near agreement with Dr. See leads me to think that they had for the most part the same orbits in view. I resolved accordingly to go over my former results with these selected orbits, and ascertain whether they still held good. I hope on making some further inquiries to be able to arrive at a fairly definite conclusion on this subject; but the inquiry is rendered difficult by a fact which I had already noticed, and which comes out very prominently when we are dealing with selected orbits only, viz., that a very large proportion of the binary stars which have satisfactory orbits have the same spectrum. This is the point which I desire to insist on in my present letter.

Of the 40 binary stars in Dr. See's list I have been able to obtain the spectra of 29 from the Draper Catalogue. Calling stars with the spectrum A, B, C, or D, *Sirian*, those with the spectrum E, F, or G, *Capellan*, and those with the spectrum H, I, K, or L, *Arcturian*, the result (discarding notes of interrogation) is 4 Sirian stars, 23 Capellan, and 2 Arcturian. These figures are out of all proportion to the frequency with which stars, and even double stars, with these kinds of spectra occur in the sky. What is the explanation? It is, I think, indicated by another circumstance. Nearly all of the 23 Capellan binaries have proper motion larger than the average, and are for that reason included in the 'Cincinnati Catalogue' for example. This raises a presumption that they are nearer to us than their compeers. But if so, why are they not brighter? If their deficiency in this respect was due to their small masses, why should the periods of revolution be so short and the angular measure of the axis-major of the ellipse so considerable? For, of course, they would not find their way into Dr. See's list if the period was very long or the axis of the ellipse too short to admit of fairly accurate measurement. We thus seem to be led by a different path to the conclusion that Capellan stars (of the same mass) are less luminous than either Sirian or Arcturian stars. However, my present object is to call attention to the fact of their remarkable preponderance, and to express a hope that astronomers will seek an explanation of it.

Yours faithfully,

Dublin, 1897, Sept. 13. W. H. S. MONCK.

On the Reduction of Observations.

GENTLEMEN,—

Some time ago attention was called in your magazine to the very common fault of using too many decimal places in computation, and in a recent note to a closely allied fault, namely the blind and implicit belief, on the part of some computers and astronomers, in the superiority of logarithmic computation under any and all circumstances. The writer says:—"The subject (use of decimal places) is by no means exhausted, although perhaps quite as much has been said as could have appropriate place, &c." I do not think that too much *can* be said on this subject, for the error is a very deeply rooted one, and can never be completely eradicated until it has been made the subject of all the ridicule it so justly deserves. It is not only the question of the mere waste of time resulting from unnecessary computation; it gives to the reduced result of the observations a spurious appearance of accuracy that generally is accepted as real by the layman and student, and may mislead for the time being those who ought to know better, particularly when the result is followed by that magic and "multitude of sins covering" expression known in the language of least squares as "a probable error." It is time that it should also be clearly appreciated, that, however valuable the least-square method of reduction may be when applied by the *proper persons* to observations made under the *proper conditions*, it is, I will venture to say, in the majority of cases in which it is used, only another example of a useless expenditure of labour, resulting in an apparent and not a real accuracy of result. A "probable error," rightly interpreted, represents not a given approach to a true value, but only a given degree of consistency among the observations themselves, arising from a more or less complete absence of sources of disturbance—a very different thing from the elimination or reduction of the sources of errors of observation, either constant or accidental.

Professor Turner has begun the campaign against these least square seven-figure logarithmic methods of obtaining scientific (?) results, in which it is hoped he will persevere until they have come to be estimated at their true value. He is being ably supported by Professor Burnham, whose recent investigations of the computed elements of certain double star-orbits demonstrate, as nothing else has done for a long time, the utter folly of expending a great amount of mathematical labour on observations which are themselves so few in number and so discordant as those from which these elements had been deduced. These efforts should certainly meet with the heartiest support from all lovers of truth and real accuracy in scientific work.

In this connection it may not be out of place to call attention to a recent glaring example of the questionable practice of giving the results of the measures of double stars, made with comparatively small instruments, to the second and even the third place of decimals. In a recent number of the 'Astronomische Nachrichten' (No. 3421, p. 214) there is a report of some recent measures on the position of the Companion of Sirius, which has been widely copied and quoted in other journals. These measures were made with a 7-inch telescope, which would have a (visual) resolving power of about $0''\cdot6$. In a paper in the Phil. Mag. (July 1897, p. 83) it has been shown that the accuracy with which a fine point or narrow line can be located with reference to the *double* cross wires of a micrometer under the most favourable conditions will not exceed 5 to 10 times the theoretical resolution. And under the conditions met with in astronomical work, in which *single* cross wires are generally used, and in which the distance between *two* objects is measured, the accuracy certainly cannot be greater than three times the resolution; *i. e.*, the error of a single double-setting will be at the very least one third the resolving power, or in the above case $0''\cdot2$. In the first series of the measurements in question the distances are given to $0''\cdot001$, quite regardless of the fact that they vary among themselves by as much as 750 times that amount. In a second series, made under what the observer considers better conditions, the mean (the individual measurements are not given) of the measurements of distance is given as

$$d=3''\cdot7575.$$

If we consider that the probable error of a single observation is 0″·2 (which would mean that there were *no* errors save the *minimum errors* of setting the cross wires on the two objects), we might fairly infer that this result (no other data being given) was the mean of 160,000 observations, the number necessary to reduce the "probable error" of the mean to 0″·0005, which is perhaps even larger than we are meant to understand as the error of this determination. It is difficult to believe that the observer would have been able to accomplish this feat in 75 minutes, the time allotted to the making of these observations.

Yerkes Observatory,
1897, July 24.

Yours faithfully,
F. L. O. Wadsworth.

Personal Equation &c.: and the Amateur Astronomer.

Gentlemen,—

Reading the gloss or comment on my letter on p. 353 by the Owner of the Oxford Note-Book, it strikes me that there is a certain amount of mutual misunderstanding between us. For so much of it as I am responsible I must express my regret that my command over the English language is, or was, not sufficient to enable me to render myself intelligible.

The Note-taker spoke of the subject of Personal Equation as being "a link between the professional astronomer and the *dilettante*." It may be that I am too thin-skinned, but this struck me as a covert sneer at that great mass of astronomical amateurs who, if incompetent to enter into the arcana of fundamental astronomy, have yet, many of them, done real and enduring service to science. I am glad to find that it had a much more restricted meaning than I attached to it. In another respect, the Owner of the Note-Book entirely misapprehends my meaning. It was to intellectual and in no sense whatever to *pecuniary* wealth that I was referring when I spoke of "the rich man's table." I have said, over and over again, publicly and privately, that I consider the salaries paid to the men who do such splendid work in our public observatories as not only miserably mean, shabby, and cheese-paring, but as utterly inadequate to the services which are rendered in return for them.

A word as to your editorial comments in your own "Notes." My meaning was simply this, that I did not see why (what appeared to me, in my ignorance, to be) such an instructive paper as that of Professor Safford should be relegated to the dilettante any more than one on the R—D correction, or on the measurement of the plates for the Astrographic Catalogue, the necessity for both of which I should be among the first, though, heartily to admit. But I would say, "these ought ye to have done, and not left the other undone." It must not be supposed for one single instant that I fail to recognize the paramount importance of eliminating every conceivable source of instrumental error, or that I do not most thoroughly appreciate the inestimable value of the

Astrographic Chart and Catalogue, which, when completed, will be one of the grandest achievements of the century, and a hundred, or even fifty, years hence will be simply priceless as the means of solution of more than one problem which is *vexata quæstio* to the astronomer of the present day.

With reference to the relations between the Professional and the Amateur Astronomer, I can scarcely imagine any sane man paying attention to such preposterous charges as those which you say have been made in one of your contemporaries in connection with our National Observatory. Even to notice them is to confer a factitious importance on them. When the navvy's wife scratched his face, and wonder was expressed that he did not retaliate, his simple remark was: "it pleases she, and it don't hurt me." *Verbum sat sapienti.*

Forest Lodge, Maresfield, Uckfield.
1897, Sept. 6.

Yours faithfully,
William Noble.

PUBLICATIONS.

Jupiter-Beobachtungen an der Manora-Sternwarte, 1895–1896.—Under the above title a really magnificent memoir on the physical aspect of Jupiter by Herr Leo Brenner has recently been published by the Vienna Academy of Sciences. The feature which entitles the work to be called "magnificent" consists of a series of very fine tinted charts of the whole surface of the planet. Concerning these it is safe to say that nothing equal to them in point of finish and quantity of detail has hitherto appeared. The charts have been constructed from drawings made at hourly intervals, but, since it would necessitate a continuous watch of nearly 10 hours to delineate the whole surface in this way on a single night, the drawings of two and sometimes three consecutive nights have been combined to furnish a complete map of the whole planet. Herr Brenner's drawings of Jupiter are well known, and as the work of constructing the charts appears to have been carefully done, the result is very fine. We have here a series of complete maps of the planet at convenient intervals throughout the apparition of 1895–6, and by means of these the changes taking place from time to time can be easily followed. Excepting perhaps from a purely pictorial point of view, it would, however, seem preferable to keep the results of each night separate, after the manner adopted by the late Mr. E. P. Sells in his charts of Jupiter, published in the 'Monthly Notices,' vol. lvii. p. 152. Herr Brenner has certainly indicated by means of brackets those parts of his maps which are based on the work of separate nights, but the drift of the equatorial surface-markings is too rapid to enable the results of two or three nights to be satisfactorily combined in this way.

Besides the charts and a number of separate drawings of the planet, the work contains a full detailed description of the general appearance of Jupiter, with micrometrical measures of the latitudes of the belts, and an extensive series of tables designed to show the identity and motions of individual markings. These tables constitute perhaps the least satisfactory portion of the present memoir. Most of the positions contained in them are only derived from drawings, and are consequently merely approximate. As the spots on Jupiter are very numerous and frequently have a close resemblance to one another, it is easy under such circumstances to make mistakes in identification. Hence it is not surprising to find that the positions of markings held to be the same are often very discordant, and that Herr Brenner sometimes comes to very anomalous conclusions relative to the motions of the spots. The apparently highly irregular motions, to which attention is frequently called, appear to be largely due to errors of observation or mistakes in identification, and the conclusions at the end of the work are vitiated in consequence [*]. Notwithstanding this defect, however, Herr Brenner's work cannot be regarded as otherwise than magnificent, and it should be in the hands of every observer or student of the planet. Only caution should be exercised before accepting the identifications of the spots. A. S. W.

NOTES.

COMET NOTES.—We continue the ephemeris of d'Arrest's Comet from Ast. Nach. 3441. It is for Paris midnight:—

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	′
Oct. 4....	4	32	58	2	45	Oct. 20....	4	24	54	5	12
8....	4	31	51	3	24	24....	4	21	34	5	42
12....	4	30	6	4	3	28....	4	17	49	6	4
16....	4	27	46	4	39	Nov. 1....	4	13	45	6	31

The comet was observed by Cerulli on Aug. 25, the R.A. being 34 seconds less than that of the ephemeris. It was extremely faint.

Comet 1897 I. (Perrine) was observed by Mr. Thome at Cordoba up to April 29 last, when it was an exceedingly faint object, only observable with great difficulty. This comet faded much more rapidly than would have been expected from its theoretical brightness.

'Bulletin Astronomique' for August contains a preliminary discussion of the orbit of Comet 1886 V. (Brooks), by Mdlle. Klumpke. This comet was discovered on April 27, 1886, and was observed in the northern hemisphere up to May 28, and in the southern hemisphere from July 3 to July 30.

In this preliminary discussion Mdlle. Klumpke only uses four observations made on April 29, May 25, July 6, July 30. From these she shows that the assumption of a parabolic orbit is inadmissible, and deduces the following elliptic elements :—

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[*] In order to realize the value of exact observations, it is only necessary to compare the discordant positions of many of the spots in the tables with the relatively very accordant ones of the "violin" and "garnet" spots on p. 19. The two last-named spots were accurately observed, and their slow, steady, drift to the westward is well shown.

T = 1886 June 7·3913 Paris M.T.

π	33° 54′ 50″	1886·0.
☊	192 37 27	
i	87 40 24	
log e	9·99857	
log q......	9·43106	
Period	745 years.	

The September number of the same publication contains some researches on the stability of comets of short period. We have in recent years seen evidence of disintegration in several cases, and M. Callandreau shows in the paper referred to that this can be accounted for by the perturbative action of the Sun and Jupiter on the different portions of the comet, without having recourse to thermal or electrical agencies.

There is also in the same number a computation of the secular variations of the orbit of Encke's Comet produced by Jupiter and Saturn. This is by M. A. Lebeuf, and the following are his results for the combined action of the two planets :—

$$\frac{de}{dt}=+1''\cdot4\,; \qquad \frac{di}{dt}=-26''\cdot1\,; \qquad \frac{d\Omega}{dt}=-36''\cdot8\,;$$

$$\frac{d\pi}{dt}=+29''\cdot0\,; \qquad \frac{d\epsilon}{dt}=-136''\cdot6.$$

A. C. D. C.

Minor Planet Notes.—Three new planets have been discovered by M. Charlois—DG, DH on Aug. 25, and DI on Aug. 27. These are the first discoveries of the present year.

The following two planets have been named :—

(354) Eleonora. (416) Vaticana.

They were discovered by M. Charlois 1893, Jan. 17, and 1896, May 4 respectively.

Planet DC, discovered by Charlois 1896, Dec. 28, has received the permanent number 425. A. C. D. C.

The Total Solar Eclipse of 1898 January 21.—Preparation and arrangements are beginning to be made for the forthcoming Indian Eclipse. We are indebted to papers by Mr. E. W. Maunder for the following *résumé* of the programme laid down for the English expeditions.

Three parties will be sent out by the Eclipse Committee.

Sir J. N. Lockyer and Mr. Fowler will be stationed near Ratnagiri on the west coast. The equipment will be chiefly spectroscopic, and will include two telescopes of nine and six inches aperture respectively, furnished with objective prisms and an integrating spectroscope having two 3-inch prisms of sixty degrees

The method of observation with all three instruments will be photographic, a long series of varying exposures being made with the objective prisms, whilst an exposure of sixty seconds will be given with the integrating spectroscope.

The second official party, consisting of the Astronomer Royal, Prof. Turner, and Dr. Common, will occupy a station, which will probably be Jeur or Marad, about 100 miles south of Poona. The Astronomer Royal will take the Thompson photoheliograph of nine inches aperture and 8 ft. 6 in. focal length, furnished with a secondary magnifier placed within the focus so as to give an image of the Sun four inches in diameter. Prof. Turner will use the double camera, which has already seen service in more than one eclipse. This consists of two square camera tubes, six feet long, one furnished with the "Abney" photographic lens of four inches aperture and 5 ft. 2 in. focal length, the other with a 4-inch object-glass, which will be supplied with a secondary magnifier so as to give an image of the Sun 1½ inches in diameter. This instrument and the Astronomer Royal's instrument will be fed by a sixteen-inch plane mirror mounted as a coelostat. Dr. Common's equipment will be the same as he took to Norway last year—a 6-inch doublet lens by Grubb, giving an image of the Sun 3½ inches in diameter, a slit spectroscope with 3½-inch lenses and two prisms of sixty degrees, and a grating spectroscope to be used without a slit. The plates for the Astronomer Royal's coronal photographs, Dr. Common's photographs with the 6-inch lens and with the grating spectroscope, are all to be of dimensions 12 × 10 inches.

The third expedition sent by the Committee will take up its position at Wardha, on the Great Indian Peninsula Railway line. Here Mr. Newall will use a large slit spectroscope in an endeavour to determine the speed of rotation of the corona by the relative displacement of its spectrum-lines east and west of the Sun, and Capt. Hills will use two spectroscopes with slits respectively tangential and radial to the Sun's limb. Dr. Copeland will be in this neighbourhood.

Some twenty or more members of the British Astronomical Association propose to go to India and will occupy a station near Nasur, on the South Mahratta Railway, 107 miles south of Poona. Prof. Michie Smith will be at Karad on the same railway-line, and Prof. Naegamvala, with his elaborate equipment and about twenty assistants, will be at Jeur on the Great Indian Peninsula Railway.

An expedition, with Prof. Campbell as chief, from the Lick Observatory has been organized. The programme of this expedition will be both spectroscopic and photographic.

TWO NEW DIVISIONS IN THE SATURNIAN RINGS.—We have received the following from Herr Brenner, at Lussinpiccolo, dated Sept. 3:—"On the 27th August I discovered two new divisions in the rings of Saturn: the one between the Manora division and the inner edge of the crape ring is identical with the Struve division, discovered in 1850, and seen again in 1887 by Struvaert.

The other division, between those of Encke and Cassini, is quite new and was never before seen. Nevertheless it was more consipcuous than the Encke division and more extended too. Mrs. Manora saw it with ease, and so did we every day (up to date) since 27th August! But I must mention that for 9 days we have had 'air 1' every night and almost every day! The best illustration to show what kind of seeing this represents is the fact that Saturn shows a *perfectly steady* image at an *altitude of only* 12°, and that powers up to 830 diameters could be employed. Besides we were able to observe these new divisions even half an hour before sunset! It is a pity that no English observer has been able to visit our observatory during the last days; for those who have not seen such a calm and transparent air would scarcely credit its possibility!"

THE CONSTANT OF ABERRATION.—Not the least interesting fact relating to the series of observations by Talcott's method which are being made at various observatories for determination of variation of latitude, is that they give as a bye-product a value of the aberration constant. Prof. Doolittle, who made such a series for latitude in 1892–93, has lately published (Ast. Journ. No. 406) the resulting value for the constant of aberration, which comes out 20″·55 ±0·01. For the following short collection of similar results we are indebted to 'Nature':—

		″
1843......	Struve	20·445
1844......	C. A. F. Peters	20·503
1849......	C. A. F. Peters	20·481
1850......	Maclear	20·531
1861......	Main	20·335
1882......	Downing............	20·378
1883......	Nyrén	20·492
1888......	Kustner	20·313
1895......	Newcomb	20·511

MR. INNES, writing from the Observatory, Cape of Good Hope, says:—"The double-star *h* 4707 = Bris. 5071, R.A. 14^h 46^m, Dec. −66° 0′, is evidently a fairly rapid binary. I swept across it recently and made a rough estimation as follows: $p=140°$, $\Delta=0''.7$, mags. 8·4 and 8·6. Previously we have:—

h 1835..........	220°	1″·5	1 *n*
h 1837..........	225	1 ·5	4 *n*
Russell 1871	Not found.		1 *n*

"The night on which Mr. Russell looked for the double was foggy; nevertheless, no doubt the star was examined but found to be single then."

FROM AN OXFORD NOTE-BOOK.

BEFORE our next number is published the Yerkes Observatory will have been formally opened. The opening ceremony was originally fixed for a date now more than a year back; but several circumstances have caused delay, one of them amounting to a sensation of considerable magnitude, viz., the falling of the movable floor for the big telescope from top to bottom of its run only a few days after it had been erected.

I have lately received some photographs of the wreckage, which give a terribly vivid notion of the disaster. One trembles to think of what might have occurred had the fastenings held a little longer—say until the floor was crowded with visitors, whose extra weight might very easily have been the determining cause of the fall. As it is, by marvellous good fortune, no one was on the floor at the time, and the damage done was only material, though sufficiently serious at that. There seems to be no doubt that the cause of the accident was careless engineering—carelessness of a very culpable kind; but for some reason the exact facts are not to be made public. By strenuous exertions the floor has been repaired with extraordinary speed, and it really seems as if a start would soon be made with the great telescope.

PROFESSOR BARNARD has indeed apparently got to work already. A copy of the 'San Francisco Chronicle' for September 1 gives the following account of a "new discovery" made by him:—

> He pointed the instrument toward the region located to the astronomer in the position of 312 degrees, distance 53 minutes. He had frequently pointed the Lick instrument at the same spot with the expectation of finding something new. To Burnham on Mount Hamilton it showed space and nothing more. He watched whole nights, but discovered nothing. He (Prof. Barnard ?) swung the giant tube toward the region, and the first discovery at the Yerkes Observatory was registered on the dial near the dome. The unknown wanderer was found near Winnecke's companion to Vega. Although it might claim some distant relationship to Vega's companion, it is too far away to be considered as physically connected with that luminary, and may have an orbit of its own. As Vega has been one of the best-observed bodies of heavenly wanderers, the discovery speaks volumes for the new lens, and also disproves the theory of Professor Lowell as to the unfavourable atmospheric conditions of the region in which the telescope is located.

IT is not very clear what exactly was found out, and so I give the extract literally, for the sake of those who are clever at guessing. The announcement may have been made more intelligibly through other channels; and if so, readers will have a key for translating the above account. As it stands, we can only surmise that the object discovered is a faint companion to some star, the designation "wanderer" not meaning that it is a planet or a comet, only that it "may have an orbit of its own." But there is an unmistakable note of satisfaction—nay almost triumph—in the account, which must have been sounded originally at headquarters; the telescope and climate have both behaved well, that is certain; and we can afford to wait patiently for further news. It is specially pleasing to know that the atmospheric conditions are good, at least at this time of year. In the winter we have already heard that it is terribly cold at Lake Geneva, and work practically impossible at times. When and where Professor

Lowell disparaged the climate I do not know, but incidentally I may say that he has been kind enough to tell us something of his opinion of Mexico and Arizona in a brief note which will appear next month.

THE phrase about "registering the first discovery on a 'dial near the dome'" is one of the most interesting bits of the puzzle. I confess I don't quite know what this is. Is it merely the tapping off of some position chronographically? Or is the dial some apparatus for publicly announcing a discovery? At Washington Professor Harkness has arranged very ingeniously for the readings of circles to be shown in dial-fashion; and some form of his apparatus may have been adopted at the Yerkes Observatory. But this is only surmise. Perhaps Professor Hale will tell us some time what this dial is.

PARADOXERS are fearfully and wonderfully made. Two of them have lately produced a volume* of 500 pages on 'Some Unrecognized Laws of Nature'—a book just the size of De Morgan's whole 'Budget of Paradoxes.' Nature is explained by means of four First Principles, viz., the laws of Persistence, Unequal Resistance, Equalization, and Reciprocity, from which follow eight propositions. Proposition II. is interesting in relation to the old problem of Mahomet's coffin. "A body attracted simultaneously by two or more bodies in opposite directions will assume a position between such bodies, and proportionally nearer to the one by which it is more strongly attracted." (No proof is offered, of course; the paradoxer is generally careful to throw the *onus probandi* on others.) These propositions are then applied to the elucidation of planetary motions as follows:—

> In the first place, Sun and Earth would attract each other, being in different states of excitation. And if Sun and Earth were not simultaneously attracted in opposite directions they would meet. But both Sun and Earth are pulled at the same time in other directions. . . . . What bodies draw them in other directions we shall not undertake to consider here. . . . .
>
> Coming back to our main argument, from the length of the Earth's orbit and the number of axial revolutions, we have deduced the diameter of the Earth's sphere of influence as opposed to that of the Sun. . . .
>
> It is hardly possible to apply this theory without being forced to the conclusion that the Sun-spots may be planets in a very low state of excitation. . . . The irregularity in shape of Sun-spots presents no difficulty to the theory that they are planets but a comparatively short distance from the Sun and revolving round it, for if thumb and forefinger be brought near each other, but without actually touching, while looking at a strong light, they will be seen as if connected by two protuberances.

It is curious that people who can calmly write down such sentences should accept anything at all from such mistaken lunatics as ordinary scientific folk must seem to them; yet the authors of this amazing production use the figures of the 'Nautical Almanac' to explain precession. I wonder how and where exactly they think we have gone wrong? Again, how comes a respectable publisher to aid and abet them, and to encourage his press-reader to the "painstaking and searching vigilance which enabled him to discover and to correct so many slips of grammar as almost to *justify in us* (the authors) the hope that none are left in the book"? (The italics are mine.)

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* 'Some Unrecognized Laws of Nature': Singer and Berens. (London: John Murray, 1897.)

De Morgan, at the end of his 'Budget of Paradoxes,' insisted that

> All is not barren: I think I shall find, on casting up, that two out of five of my paradoxers are not to be utterly condemned. Among the better lot will be found all gradations of merit; at the same time, as was remarked on quite a different subject, there may be little to choose between the last of the saved and the first of the lost.

I don't know whether he meant to include in the gradations of merit possibilities of amusement; if so, Messrs. Singer and Berens ought to come rather high up, and would have been higher still if it had not been for that proof-reader. But I am sorry to see that the book was printed at the Oxford University Press.

At the moment of sending these notes for press (Sept. 29^d 9^h G.M.T.), I have just received a letter enclosing the following circular, which I think appeals sufficiently strongly to our sympathies to be printed at the earliest possible moment:—

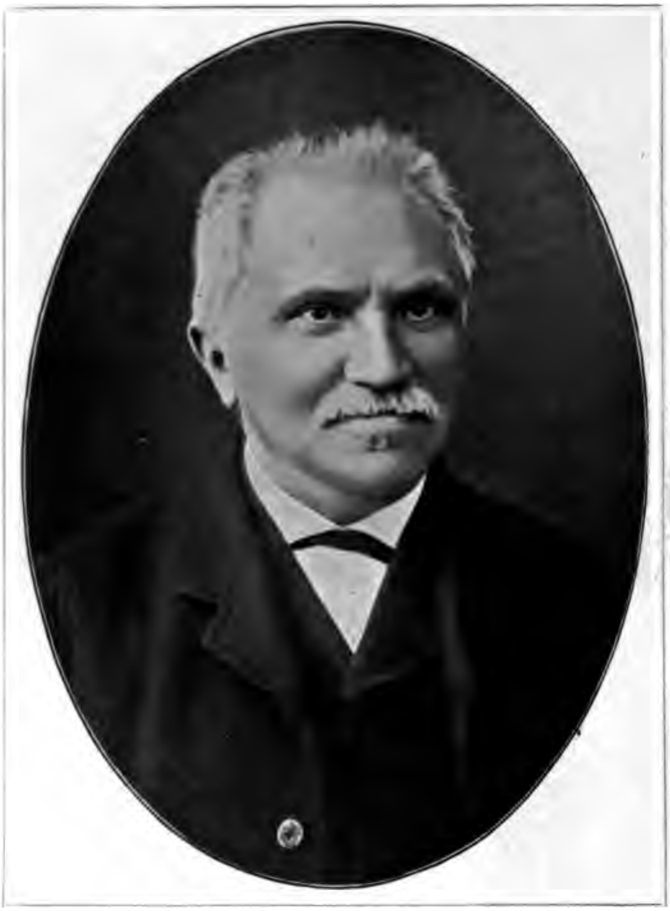
Loss of Library by Fire.

On September 14, 1897, at 1.30 A.M., the Grand Canyon Hotel in Flagstaff was discovered to be on fire, and within 20 minutes from the first alarm the entire building was in flames. Owing to the headway already made before the alarm was given, and the insufficiency of the water supply, and the consequent weakness of the fire department, all hope of saving the contents of the building was at once given up. In this Hotel had been the Office of Mr. Cogshall and myself ever since the beginning of our work with the Lowell Observatory, August 1, 1896, and our apartments contained all the mathematical, astronomical, physical, and other books which I had brought with me from Chicago, or since purchased; besides various precious papers, letters, manuscripts, pictures, &c., and the records of our work on the double stars of the southern hemisphere. The night being unfavourable for observations we had retired, and in the hurried moments of darkness and confusion attending the evacuation of the burning building, we were barely able to save the records of the Observatory, the general manuscript catalogue of all double stars within 75° of the south pole, and a few other works such as the 'Mécanique Céleste'—everything else, the library of books, the manuscripts, letters, pictures, personal effects, being a total loss.

As this destruction of the library will necessitate the formation of a new one, I beg to state that I am desirous of restoring first the astronomical works relating to the double stars of the southern hemisphere. If friends or other men of science with whom I have exchanged publications should have copies or reprints of their works which they would feel disposed to offer, I need hardly add that they would prove very useful and be received with grateful appreciation.

The writer hopes before long to offer to his correspondents another contribution, namely, the discoveries and measures of double and multiple stars in the southern hemisphere made at the Lowell Observatory, which, fortunately, he was able to rescue from the flames. T. J. J. See.

Lowell Observatory,
Flagstaff, Arizona, Sept. 16, 1897.



ALBERT MARTH.

From a photograph by P. Risse, Duisburg.

[illegible]

THE OBSERVATORY,

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On the Climatic Causes of the Removal of the Lowell Observatory to and from Mexico.

Climate, viewed telescopically, is perhaps the most pregnant of all the astronomical possibilities of the day; to it are already due the recent results in planetary work which seem so incredible to those who have not seen the phenomena, and the vista widens on acquaintance. To give, therefore, in answer to a request of the Editors of the 'Observatory,' the reasons for the removal of this Observatory to and from Mexico, may, as they suggest, be of interest to observers generally—the more so that the conditions at Tacubaya have opened our eyes to certain further necessary details in our search for the great desideratum.

Through three media comes to us our knowledge of ultra-terrestrial things—the air, the instrument, and the observer; and so skilled have opticians become in these latter days that it is to the performance of what stands without the instrument at its big and little ends respectively, not to the instrument itself, that vision or non-vision is chiefly due. As to what stands at the little end, two points alone need be touched upon here—points based upon some experience in the matter: first, that eyes differ very little, brains very much; and secondly, that brains differ not only in penetration with all that that term includes, but, for psychic reasons, in their openness to certain broad classes of perception. In air which falls below unmistakable revelation, an acute detector of faint points of light will remain blind to planetary detail which to another is much the more obvious of the two phenomena, and each passes for a visionary to his neighbour until improved atmospheric conditions prove each to be right. Curious instances of this, even as between different classes of detail, as that between the double canals of Mars and the canals in the dark regions of the same planet, have come under my immediate notice. Practice has shown me that observers are more likely to be prejudiced against, than for one another's discoveries, but that improvement in the seeing almost invariably corroborates what one had been tempted to doubt.

But to be sure of his facts even an acute observer must have good air; and what an "open sesame" good air is can be appreciated only by those who have tried it. The wonder is that we can ever perceive anything from the pit of that vast seething cauldron we call the atmosphere. Thanks to Mr. Douglass, however, we are beginning to see daylight in the theoretic conditions necessary to secure the best place at the bottom of our ocean. From what he has learned we now know that the currents in the air are composed of waves of condensation and rarefaction which can be seen and measured; that these waves refract the incident rays in a manner which is calculable; and that in consequence certain currents are bad, certain others comparatively harmless. We are beginning, in short, to unravel the tangled skein furnished us by nature. Into this, which to us is the most interesting part of the subject, I cannot here go. I shall but mention a single point to show its trend. The controversy between large and small apertures turns out to be a question of the wave-length of these waves. If the wave-length be more than double the aperture of the smaller glass, we get bodily motion of image without confusion, while in the larger one we get blurring of detail. The proof of this is evident. Sometimes, therefore, the smaller glass shows the better definition; sometimes the larger. The wave-lengths vary in different currents from half an inch to some feet.

Here, however, I must confine myself to the practical side of the subject, and give of this an illustrative example, the difference between Flagstaff, Arizona, and Tacubaya, Mexico.

Water-vapour is the great telescopic bugbear. For water-vapour is to the air what emotion is to a character: the great unsettler of equilibrium. If nothing occur to cause disturbance, water-vapour is no bar to sight, some of the best definition being got through fog; but the moment anything upsets the equilibrium the presence of water in the air increases the catastrophe, owing to the thermic qualities it possesses. To secure, therefore, a climate as continuously good as possible, we must go where water is scarce, or, in other words, to some part or other of the two great desert belts that girdle the earth: the northern one consisting of the great American desert, the desert of Sahara, the Arabian desert, and the desert of Gobi; while the southern one is made up chiefly of the South American deserts to the west of the Andes in Peru, Bolivia, and Chili, and parts of the Australian Continent.

In the next place, the less air we can have above us the better. Consequently we must select as elevated portions of these deserts as we can. On the other hand, these places should be of the nature of plateaux, not of peaks; for the peaks themselves cause convectional currents which are disastrous for a greater or less portion of the twenty-four hours, and, furthermore, bend and break the currents that pass over them.

Several such high plateaux occur in the great American desert, notably one in which Flagstaff is placed, one near Zacatecas, Mexico, and one around the City of Mexico itself. These all stand 7000 or 8000 feet above sea-level. Plateaux lying far north in the desert belt are to be avoided, and similarly those lying too far south, inasmuch as the desert belt itself shifts, following the Sun, and thus only the heart of the desert, and sometimes not even that, stays such throughout the year.

Lastly a desert pure and simple is too much heated by day, producing very bad convectional currents in the daytime, lasting on into the early night. To secure uniformity of condition we must, therefore, have a large oasis in the midst of a desert. All these conditions are fulfilled by the high pine-plateau of Northern Arizona, in which Flagstaff is situated; but here, as elsewhere, latitude leaves something to be desired, for while Flagstaff has a slight rainy season in summer common to the whole tableland, it lies far enough north to border the edge of the great cyclonic movement in the north temperate zone in winter. Not really in the movement, it is sometimes reached by the outlying branches of it. The neighbourhood of the City of Mexico is, on the other hand, to the south of such disturbance. This was one of the reasons for moving south for the winter.

Preliminary observations had previously been made there by Mr. Douglass with a 6-inch, which indicated fairly good seeing. And this is what the seeing proved to be; but in addition it turned out to be capricious. Two causes combined to this result—one general, the other local.

The general adverse condition was that Tacubaya lies too near the southern edge of the desert belt, and in consequence is subject to surface-winds more or less northerly. If we look at a chart of the winds of the world, we shall see that while the prevailing winds at Flagstaff are northerly in winter, they are southerly in summer, while at the centre of Mexico they are variable the year round. These charts give the surface-winds. The upper currents are progressing from the south northward all the time to descend to the surface in the temperate zone. Now to insure steadiness it would seem that we must have agreement in direction between the upper and the under currents. And this proved to be necessary experimentally. On the whole when the surface-wind was from the south we had good seeing; when from the north, poor.

But there was another reason for the uncertainty of the conditions in the valley which introduces an interesting detail into atmosphere hunting: the local topography. Tacubaya, the village above which the observatory stood, lies on the western slope of the valley, about two hundred feet higher than the City of Mexico, whose cathedral and spires it overlooks. On all sides rises a rampart of hills from one to three thousand feet high, towered over to the south by the snow-clad Itlacihuatl and Popocatapetl. In the daytime the valley gets heated, and the air draws in to the point of greatest heating, which, when other particular cause is absent, turns with the Sun. During the day the result is a wind from the south or south-west, but near sunset it shifts to north-west, and the seeing at once goes off. Nor does it recover till late into the night. This is when it is good; if it be bad, we have a north wind continuously, due to a storm in the temperate regions, of which we get but the whisk of the tail.

At Flagstaff, on the other hand, the local conditions are of the best. The long plateau stretches off to the southward, falling gently for a hundred miles, while on the north stand the San Francisco peaks in rampart six thousand feet above the observatory site. When the air moves of a piece above and below, as it does when the surface-wind comes from the south and west, we have generally good definition. When the surface-wind comes from over the peaks we have poor seeing.

By the move to Mexico, then, I think that we have added to the necessary details of condition this further one, the necessity for agreement in direction between the upper and the under atmospheric currents; and agreement in direction carries with it as much agreement in temperature as is attainable.

Lowell Observatory, PERCIVAL LOWELL.
1897, Sept.

On the Photography of Planetary Surfaces.

[Continued from p. 370.]

II. Turning next to the question of the best optical form of instrument (refracting or reflecting) and to the best form (mechanical) of mounting, we find several other points worthy of careful consideration. In regard to the question of refractor or reflector, I am of the opinion that the former type of instrument is decidedly superior to the latter for general photographic research. In addition to the remarks made in a former paper (A. N. no. 3439; and 'Knowledge,' Aug. & Sept. 1897) on this point, it may be added that with such a large ratio of focal length to aperture as is here advocated for the planets, the chromatic and spherical aberrations may be almost perfectly corrected over the whole range of wave-lengths to which the photographic plate is sensitive. In regard to the many statements that have lately been made about the relative sizes of the images of points (stars, for examples) as formed by the reflector and refractor, it seems proper to point out in this connection that when the instruments are properly corrected for spherical aberration there is practically very little difference, aperture for aperture, between the two types of instrument, when used either visually or photographically, provided only the refractor is so corrected that the wave-lengths which are brought to minimum focus are those which correspond to maximum sensitiveness of the eye and photographic plate respectively. For these wave-lengths play by far the larger part in determining the character of what may be called the "effective diffraction image" of a point, which is a very different thing from the real diffraction image formed by the superposition of the individual patterns due to all the radiations, of wave-length ranging (perhaps) from 0 to ∞, emanating from the point whose image is under consideration. The rapid falling-off in sensitiveness on each side of the maximum makes those wave-lengths which do not come to the minimum focus (of the refractor) of comparatively little importance so far as their effect in determining the distribution in intensity in this "effective image" is concerned. Hence, as said before, the "width" and character of this image is very nearly the same in both refractor and reflector. This point will be dealt with more at length in a subsequent paper, in which I shall speak of some applications of the study of these "effective" and real diffraction images in the measurement of stellar diameters, stellar magnitudes, and (given a sufficiently sensitive radiometer) of stellar temperatures.

As regards the form of mounting best adapted to the purposes of planetary photography, there are two or three conditions which must be carefully fulfilled if we are to realize to the full the advantages of the intermittent exposure method and attain the highest possible accuracy of "following." In the first place, the telescope used for following should be of at least as large an aperture, and preferably much larger than the photographic telescope. The two objectives ought to be of nearly the same focal length (to eliminate as far as possible the effects of flexure) and mounted rigidly together in the same tube (like Prof. Vogel's instrument at Potsdam), and the mounting (of whatever form it may be) ought to be unusually stiff and massive. The slow motions ought all to be of the most sensitive and accurate character and actuated by separate small electric motors; and in the opinion of the writer the clock ought also to be electrically controlled and driven ('Astronomy and Astro-Physics,' April 1894, p. 265; the 'Astrophysical Journal,' March 1895, p. 169). The writer would be inclined to make use of the regular form of equatorial when there is one already at hand (like the 40-inch) sufficiently firm and massive to carry a photographic telescope of 9 to 10 inches aperture and 60 to 70 feet focal length, with the requisite steadiness. But if an instrument were to be built especially for the purpose there is no doubt that some form of coudé mounting would be far preferable, on account of diminished first cost and also because of its greater steadiness. The only objection to the latter form of mounting is the inconvenience of arranging two telescopes (photographic and guiding) of nearly the same size side by side. Of the various forms of coudé mountings, the simplest and best for this purpose is the polar siderostat, or "siderostatic telescope" of Sir Howard Grubb (Proc. Roy. Soc. Dublin, vol. ii. p. 362), modified to send the light down the polar axis instead of up, an arrangement which has the great advantage over the usual one that for this latitude (about 45°) the mirror is nearly vertical for all objects of small declination. (For drawings and discussions of this form of mounting see previous papers in 'Astrophysical Journal,' vol. iv. pp. 241 and 272, figs. 2, 3, and 4, and vol. v. p. 139, fig. 6, &c.) For the purpose of planetary photography with an aperture of 10 inches with guiding telescope of from 12 to 15 inches, a flat of from 25 to 30 inches diameter would be needed, the two telescopes being arranged side by side in the plane containing the polar and declination axes so as to best utilize the full surface of the mirror. An instrument of this design would be exceedingly stiff and rigid in spite of its great focal length, and could be very accurately controlled by the electrical devices already referred to. It might be remarked in passing that if questions of expense had to be considered a single lens (of this small angular aperture) would be practically as good for the guiding telescope as the far more expensive achromatic. Indeed such a lens might almost be good enough for the photographic objective if carefully corrected for spherical aberration.

There are a number of minor points connected with the optical and mechanical design of instruments for planetary photography, which have all been carefully considered, but which for the most part it is unnecessary to enter into here. One thing which deserves the most careful consideration is the exposing-shutter which has to be used in the intermittent exposure method. This should be made so as to work with great rapidity and yet without the slightest jar, to operate in such a way that all parts of the planet's disk get the same exposure, to open and close successively any number of times without any intermediate resetting, and, finally, should be provided with a simple device whereby the total exposure time is automatically registered. These somewhat difficult conditions would seem to be very satisfactorily met by a light blackened aluminium disk about three inches in diameter, which is pivoted just in front of the focal plane, and which has, near its outer periphery, a series of equidistant slots with radial sides, the distance between the edges of the slots being just equal to the width of the slots themselves. This disk turns just in front of a fixed diaphragm, behind which is the plate in which is pierced a circular hole just a little less in diameter than the mean width of the slots and a little larger than the image of the planet to be photographed. The rotating disk has a spiral spring or weight attached to its axis, and is provided with a series of teeth on its outer edge. A simple escapement device allows the wheel to rotate a distance just equal to the width of the slots every time an electric or pneumatic key held in the hand of the observer at the guiding telescope is pressed. This same key operates the starting and stopping button of a stop-watch (of that pattern in which the hands simply start and stop and do not return to zero), which is placed at some convenient position near the observer. The exposing disk and watch are of course so adjusted that in the beginning the watch stands at zero when the opening in the diaphragm is closed. All that the observer has to do is to close the key a number of times in succession at the beginning and end of the intervals of good seeing, until the total requisite exposure-time, as indicated by the face of the stop-watch, has been attained. Since some intervals of good seeing may not last more than one-fourth to one-fifth of a second, the disk of the exposing-shutter should be so light and the spring or weight which actuates it so strong that the actual time of full opening or closing does not occupy more than $\frac{1}{20}$ second. For the same reason the stop-watch should be capable of registering tenths and, better, twentieths of a second.

The principal conclusions which have been reached in this paper in regard to planetary photography may be briefly summarized as follows:—

(1) It is much more difficult to obtain good photographs of the planets than it is (*a*) of the nebulæ or comets, or (*b*) of the surface of the Sun and Moon: (*a*) because the detail is finer, (*b*) because the time of exposure is longer.

(2) To avoid the effects of atmospheric disturbances, which for a given aperture are considered to be the main cause of the inferiority of the photograph compared with what is observed visually, it is proposed to use a method of intermittent exposure instead of the continuous exposure heretofore employed.

(3) The best ratio of aperture to focal length (direct or equivalent) is that which is just sufficient to attain photographic resolution. For ordinary plates this ratio is about 1 to 80; for fine-grained plates it is greater (about 1 to 40); for very coarse-grained ones less (about 1 to 120).

(4) In photographing planets we need a certain minimum focal length, actual or equivalent, in order to obtain images of a certain initial size. This minimum focal length should not be less than 60 to 70 feet.

(5) It is better to use a long-focus objective and photograph directly in the focal plane than it is to obtain the desired size by use of an enlarging-lens.

(6) The conditions specified in (3) and (4) determine the *minimum* linear aperture which should be used. Under the conditions specified in (4) this should not be less than nine to ten inches.

(7) The *maximum* linear aperture is determined principally by the degree of accuracy which we can attain in "following." At present the maximum aperture of which we can utilize photographically the full resolving power is certainly not greater than ten inches, under the very best conditions, and not greater than six inches under very good ones. When we have succeeded in utilizing the full resolving and delineating powers of these apertures it will be time enough to consider how to overcome the

greatly increased difficulties due both to atmospheric and mechanical causes, attendant upon the employment of larger instruments.

(8) The total time employed in making an exposure should not exceed a certain maximum limit, which depends on the position of the planet and on the linear aperture employed. For a 10-inch aperture, and at the time of nearest approach to the Earth, the exposure time (beginning to end) should not exceed three minutes and forty seconds in the case of Mars, two minutes in the case of Saturn, nor 45 seconds in the case of Jupiter if full photographic resolution is to be realized. This condition alone precludes the use of such great enlargements and long exposures as have been employed in some cases in the past.

(9) Because of the limited range of wave-lengths to which the photographic plate is sensitive, and because of the small angular aperture and comparatively small linear aperture of the instruments necessary for the purpose of planetary photography, refracting telescopes are more efficient than reflectors, and are greatly to be preferred to the latter also on the score of stability.

(10) In order to utilize to the full the advantages of intermittent exposure method and reduce the difficulties of "following" to a minimum, we must employ for a guiding telescope one having an aperture at least equal to that of the photographic objective, and preferably much larger. The photographic and the guiding telescopes should be rigidly mounted side by side in one tube.

(11) The regular equatorial mounting can be used when one of sufficient size and stability is available, but the polar siderostat form, arranged to send the light down the axis, is considered best.

(12) The driving clock should be electrically controlled and driven, the slow motions very sensitive and accurate and actuated by electric motors, &c., &c.

(13) A special exposing-shutter, so arranged in conjunction with a stop-watch as to automatically register the total exposure time, should be used.

Conclusion.

With reference to present available equipment at this Observatory, these conditions would be best fulfilled by attaching to one side of the cell, for the 40-inch objective, a photographic objective of about nine inches aperture and of about the same focal length as the former, *i. e.* about 62 feet. This would give very nearly the right ratio of aperture to focal length (1 to 80) for fast Seed plates, and would give images of Mars at time of nearest approach to the Earth a little less than 0·1 inch in diameter, and of Jupiter and Saturn (ring system) about $\frac{1}{5}$ inch in diameter. These diameters are a little less than those which have been mentioned as a minimum limit. Of course an enlarging-lens could be used to give a larger image, in which case also a larger size objective could be used, while preserving the same ratio of focal length to aperture; but I do not believe this would be advisable. If, as in this case, we are limited to a certain focal distance (between objective and photographic plate) by mechanical or other considerations, I believe it is better to use a photographic objective just large enough to give photographic resolution at that distance than it is to use a larger one and increase the equivalent focal length by use of an enlarging-lens. In other words, I believe better results will be obtained by using a 9-inch photographic objective attached to the 40-inch, which would be used as a guiding telescope, than would be obtained with the reverse arrangement, *i. e.* with the 40-inch as a photographic objective (granting a third correcting-lens for this purpose should be provided) and the 9-inch as a guiding-telescope. For if in the second case we should photograph the direct image without enlargement, the limiting photographic resolution would be no greater than when we used the 9-inch (granting that the value of e is, in each case, 0·01 mm., as before assumed). The larger aperture would give increased brightness it is true, but the greater effect of atmospheric disturbances on the instrument of large aperture would make the intervals of good seeing shorter and less frequent than with the small aperture. It might therefore easily happen that, on the intermittent exposure plan of working, the actual time required to obtain a full exposure would be greater in the case of the instrument of larger angular aperture.

Considering the difficulties of the problem of planetary photography, and considering, too, how often certain conditions that must be fulfilled to insure a full measure of success have been violated in the past (particularly (3), (5), (8), and (10)), it is not surprising, perhaps, that better results have not been heretofore obtained in this field of work. Whether the general plan of working proposed in this paper will lead to better results or not is a question for future experiments to more conclusively demonstrate or disprove. Whether we ultimately succeed in photographically attaining the full resolving power of our instruments or not, we will at least always have the satisfaction of knowing that the photographic plate will never record details that it is beyond the power of our instruments to separate, which unfortunately is more than can be said of some recently published drawings of the planets.

Yerkes Observatory,
July 1897.

F. L. O. Wadsworth.

[*Note added August* 28.—Since this paper was written my attention has been called to the fact that the essential feature of the proposed method of procedure in the case of solar photography described on page 338 was suggested some time ago by Lohse ('Beobachtungen auf der Sternwarte zu Bothkamp,' Heft i. part 2: "Ueber photographische Aufnahmen der Sonne," p. 78). The method then was only suggested, but it has doubtless since been actually used. In this connection it is, perhaps, proper to point out that the full value of this method of working will only be realized when the aperture of the instrument used in noting the moments of good definition is at least as large as (and preferably larger than that of) the photographic objective. When the former is smaller it would be more difficult to determine the most favourable moments for exposing, because the definition in general would be much better in the observing telescope than in the (larger) photographic instrument.]

Is the Spectrum of β Lyræ of Magnetic Origin?

THERE seems at least a possibility of getting an answer to this question by the simple expedient of examining the dispersed rays of the star with a Nicol prism. A few words will suffice to make clear the motives for instituting the trial.

Astrophysicists generally, and especially those whose curiosity had been tormented by the puzzle presented to them in Nova Aurigæ, must have felt a thrill of excitement at the announcement, last December, of Dr. Zeeman's discovery regarding the modifications, through magnetic influence, of spectral lines. Essentially, it amounts to this:—Rays of monochromatic light from a source placed in a powerful magnetic field are brightened, broadened, and broken up into groups. In other words, they present just such appearances as were much more conspicuously visible in the spectrum of the "temporary" of 1892. Can they, in both cases, be attributed to the same cause? We shall see.

The results obtained by Dr. Zeeman * had been anticipated on abstract grounds by Professor Lorentz. His theory, moreover, required that the magnetically-widened lines should be circularly polarized in opposite senses on their opposite edges, and the requirement was perfectly complied with. This consilience of fact with pioneering thought is one of the most curious felicities of verification on record. The multiplication of lines proved more difficult to observe, and demanded a more intense field than their mere distension. Dr. Zeeman, however, succeeded in splitting the blue line of cadmium by turns into a doublet and a triplet, according as the light was transmitted along or across the lines of force. This, too, was in precise accordance with theory, no less than the attendant effects of polarization, which, as displayed in the triplet, were of peculiar interest. The components being polarized in rectangular planes, the rotation of a Nicol in front of the grating sufficed alternately to quench the middle line and its satellites, apparent singleness resulting from a position of the analyzing prism with its plane of polarization vertical, while duplicity ensued upon rotation through ninety degrees.

Cosmical applications of this new and beautiful criterion seem full of promise, and temporary stars especially invite its use to determine possibly the nature of their outbursts. The pity is that it has become available too late for Nova Aurigæ, by recent accounts sunk to very near extinction. An analogous object is, however, always at hand in β Lyræ, the *stable* light-fluctuations of which are accompanied by spectral changes not unlikely to declare themselves as "Zeeman phenomena." To decide the point, the first preliminary would be to examine with a Nicol the broad bright lines seen double about the epochs of secondary minimum and maximum. Typical instances are F and D_3. If magnetically affected, their opposite borders should alternately vanish as the prism is made to perform a quarter of a revolution. It must be added that, in the case of circular polarization, the interposition of a mica-plate of suitable thickness would be indispensable to the success of the experiment. Considering the wide range of its implications, it appears to be well worth trying.

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* 'Philosophical Magazine,' March, July, September, 1897.

A. M. Clerke.

The Rate Adjustment of Pendulum Clocks.

Having had on numerous occasions in past years to set up or adjust clocks to time, in ignorance of the rate or of the change of rate that would be produced by one turn of the pendulum nut, I arranged a plan to facilitate the operation of timing, by which, as soon as a rate had been found, the necessary correction to length, in parts of an inch, could be at once known. There is in the method nothing new; it consists simply in an endeavour to put known relations into a more convenient shape for practical use.

When we are confronted with a pendulum of unknown length, although unaware of the length of simple pendulum to which it corresponds, we can easily count the number of single vibrations that it makes in a given time, say one minute, which involves the length, and, coupled with a knowledge of the pitch of the pendulum screw, affords all the information required.

For any pendulum of length l, and time of vibration t,

$$dl = 2l \cdot \frac{dt}{t}.$$

The length of the mean solar seconds pendulum being taken $= 39{\cdot}14$ inches, put for l its equivalent, $\frac{39{\cdot}14 \times 60^2}{v^2}$, in which v is the number of single vibrations made by l in one minute. Adopting for the unit of time t, one day expressed in seconds, dt will represent the daily rate. Finally, considering the change of length produced by turning the pendulum nut to be equivalent to the hange of length of the corresponding simple pendulum,

$$dl = \left\{ \begin{array}{c} \text{the alteration of length (parts of} \\ \text{an inch) for a given daily rate} \end{array} \right\} = \frac{\text{the daily rate in seconds}}{v^2} \times 3{\cdot}262.$$

For rate expressed in minutes, substitute for 3·262 the factor 195·7.

Thus counting for any pendulum the number of single vibrations made in one minute, the correction to the length corresponding to a given rate is readily found. Or also, for any definite alteration of length the corresponding change of rate, either in seconds or minutes. For clocks not showing seconds on the dial, the vibrations of the pendulum may be counted by use of a watch having a seconds hand.

So far the general case. But when the vibrations are counted, there remains the inconvenience of a further little numerical operation. For my own use I therefore prepared a table such as is given in an extended form in Table II., which I employed with advantage for many years, since, in default of any knowledge of the simple pendulum length, it became necessary only *to count the number of single vibrations made by any pendulum in one minute to be able at once to ascertain the correction to length for any given rate.* But on the present occasion I have thought that it might be useful to precede that table by one applying more particularly to clocks that beat seconds, or some multiple or aliquot part thereof, which being shown on the dial, in the case of a turret-clock on an internal dial, allows the rate to be reckoned in seconds. The values of Table II. were easily made available for seconds clocks by dividing them by 60, but something yet more ready for such clocks seemed not undesirable. Such information is given in Table I., values for a three-seconds pendulum being included therein, since the 'Horological Journal,' vol. ix. page 44, contains a description of a turret-clock, erected at New York, said to have such a pendulum. Indeed turret-clocks having pendulums vibrating in four seconds are said to have been also made.

TABLE I.—For Clocks possessing a Seconds Hand.

Single vibrations made in one minute.	Time of vibration in seconds.	Daily rate in seconds.						Length of simple pendulum, in inches.
		10	20	30	40	50	60	
		Corresponding correction to length, in parts of an inch.						
20	3	0·082	0·163	0·245	0·326	0·408	0·489	352·26
30	2	0·036	0·072	0·109	0·145	0·181	0·217	156·56
60	1	0·009	0·018	0·027	0·036	0·045	0·054	39·14
120	½	0·002	0·005	0·007	0·009	0·011	0·014	9·78
240	¼	0·001	0·001	0·002	0·002	0·003	0·003	2·45

The corrections to length given in the table apply to the mean solar pendulum, but are practically applicable also to clocks regulated to sidereal time. The amount of alteration required to correct a given rate, or conversely for a given alteration the corresponding change of rate, is at once seen. The change of rate due to one turn of the pendulum nut may also be found *. Thus if the thread of the screw be such that one turn of the nut changes the length by 0·027 inch, this will be seen to correspond in a seconds pendulum to a change of 30 seconds in daily rate, to a quarter of this amount in a two-seconds pendulum, and to four times the amount in a half-seconds pendulum. For pendulums longer than a seconds pendulum, such as are found in turret-clocks, the screw would, however, be a much coarser one, having a thread two or three times as wide as that mentioned.

Many turret-clocks have no internal seconds dial for rating purposes, in some cases the pendulum-beat is fractional. For these, and also smaller clocks, showing only minutes, the rate can be known only in minutes or parts of a minute by estimation from the minute hand. For such clocks Table II. (p. 414) gives for different pendulums the correction to length corresponding to one minute of daily rate. Employing, as before mentioned, a watch to count the number of single vibrations made by any unknown pendulum in one minute—the only requirement—the correction to length for a given rate can be readily proportioned from the correction due to one minute of daily rate, furnished by the Table.

The correction to the length can be taken, for practical purposes, as in each case proportional to the rate. Thus, if a clock having a pendulum beating 100 times in a minute is found to lose 5 minutes daily, the pendulum requires to be shortened by 0·098 inch. Or, if for the same pendulum one thread of the screw =0·015 inch, one turn of the pendulum nut should change the daily rate by $\frac{3}{4}$ of a minute. For pendulums shorter than a seconds pendulum the correction to length for values of "vibrations in one minute" not contained in the table may be taken, without sensible error, as proportional thereto. In many small clocks the pendulum bob is fixed to the rod, the adjustment for rate being made by action on the pendulum spring.

When about to bring a clock to time it is not necessary to rate it at first for any long period. A very good idea of the rate may be usually formed after the clock has been going a few hours, when an approximate correction may be made, leaving the adjustment to be completed after further trial. For the final adjustment it may be well to note the rate for several successive days, say for a week, and make correction for the average rate, especially in the case of a turret-clock showing only minutes. When clocks having

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* The length of one thread of a pendulum screw may be conveniently found by measuring with a pair of compasses the length of several threads and dividing accordingly, or a rubbing of a small portion of the screw-stalk may be taken and measured.

TABLE II.—For Clocks having no Seconds Hand.

Single vibrations made in one minute.	Correction to length in parts of an inch for daily rate of one minute.	Length of simple pendulum in inches.
20	0·4893	352·3
30	0·2174	156·6
40	0·1223	88·1
48	0·0849	61·2
60	0·0544	39·1
65	0·0463	33·4
70	0·0399	28·8
75	0·0348	25·0
80	0·0306	22·0
85	0·0271	19·5
90	0·0242	17·4
100	0·0196	14·1
110	0·0162	11·6
120	0·0136	9·8
140	0·0100	7·2
160	0·0076	5·5
180	0·0060	4·3
200	0·0049	3·5
240	0·0034	2·4

large and heavy pendulums, such as turret-clocks, have been once brought to a close rate, it is convenient not further to disturb the pendulum, but rather to add to or remove from the bob, as may be necessary, small subsidiary weights. In some clocks of inferior manufacture it may happen that the pendulum nut does not turn truly square to the stalk, so that a small alteration of the nut may not always be found to produce the due change of rate.

Clock-movements are used not only for purposes of timekeeping, but frequently also as a motive power. In all cases, however, in which such movements are regulated by an ordinary pendulum, the information here given may prove to be of some service.

W. ELLIS.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Copernicus and the Phases of Mercury and Venus.

GENTLEMEN,—

It is extraordinary how often it has been stated in astronomical books that it was urged against Copernicus that if his theory were true, the inferior planets ought to exhibit phases similar to those of the Moon; and that he replied that if they were seen more distinctly this would probably be found to be the case. De Morgan pointed out ('Monthly Notices of the R.A.S.,' vii. 290) that there was no real evidence for this anecdote, the earliest mention of which that he could find being in Keill's 'Lectures' (2nd edition, 1730, p. 163). Nevertheless it has frequently been repeated since, amongst others by Mr. Chambers in his widely-read 'Handbook of Astronomy' (4th edition, vol. i. p. 105, note), who remarks: "se non è vero, è [molto] ben trovato." This expression of Bruno, which has passed into a proverb, is surely not applicable here; for a very little consideration will show that the inferior planets, if opaque bodies receiving their light from the Sun, would exhibit phases according to either the Ptolemaic or Copernican theory. This was pointed out by Rutherforth in his 'System of Natural Philosophy,' which was published at Cambridge in 1748, where he remarks (vol. ii. p. 782): "it is very strange that a defender of Ptolemy's system should make such an objection as this is to Copernicus's system, and not less strange that Copernicus should make this answer." It does not seem to have occurred to Rutherforth how apocryphal the story was, which he probably obtained either from Keill's 'Lectures,' or from Robert Smith's well-known 'Compleat System of Opticks,' 1738, vol. ii. p. 415. It is evident that, according to any theory of their motions, the planets must exhibit phases unless the Earth were not only the centre of their motions, but also their source of light, or unless the planets were either self-luminous or saturated by the solar light, which is what Copernicus (as is pointed out by De Morgan) probably believed. The phases would not, indeed, according to the Ptolemaic theory be the same, or have the same succession as they actually have; but of course Copernicus knew nothing of these, and the anecdote is that he spoke with regard to the planets exhibiting any phases at all.

Without mentioning the supposed answer of Copernicus to an hypothetical objector, no less distinguished a writer than the present President of the R. A. S. seems to have fallen into the same misconception. At p. 141 of Sir Robert Ball's 'Story of the Heavens,' we read: "It is curious to speculate on what might have been the history of astronomy had Venus only been as large as Jupiter. Were every one able to see the crescent form without a telescope, it would then have been an elementary and almost obvious truth that Venus must be a dark body revolving round the Sun." A dark or opaque body certainly; but her crescent form would not prove that she revolved round the Sun, only that she derived her light from him, and she would from time to time assume that form, if both revolved round the Earth in different periods, according to the Ptolemaic hypothesis.

Yours faithfully,

Blackheath, 1897, August 18. W. T. Lynn.

On a Point in Reflexion Observations.

GENTLEMEN,—

I regret to say that I appear in one respect to have fallen into some error in my Reminiscences, No. II., 'Observatory' for October last, page 385. Oral tradition is uncertain, and I have either misunderstood what was told to me many years ago, or given a mental twist thereto that has led to my stating apparently just the opposite to what I should have said. In speaking of the tilt of the string of images in the telescope, in the observation of a star by reflection, consequent on the mercury trough being set askew, I said that I believed it to have been in Pond's time a tradition that as good, if not a better, bisection could under such circumstances be made. I have since found some remarks by Pond bearing on this point. They are contained in a paper "On the Latitude of the Royal Observatory of Greenwich," read before the Royal Astronomical Society on November 11, 1825, and run as follows:—"The vibrations of the mercury in a longitudinal trough form an elongated image of the star in the direction of the wire: it appears like a succession of stars, which become smaller and smaller as they recede from the central undefined mass; and these stars are threaded like beads on the wire in a manner extremely favourable to exact bisection. The observer can immediately perceive if the trough be not placed as it ought to be, exactly parallel to the plane of the instrument, as in this case the line of small stars is formed transversely on the wire, and he must content himself with bisecting the central mass of light, which is much less favourable to exactness." I can only say, in explanation of the mistake, that I seem to have become mystified from the circumstance that, having myself seen, with the old mural circle, the effect of a slight tilt of the mercury trough, it appeared, I remember, to me that the observation was not thereby rendered more difficult, although I had no great experience of the matter, since one did not by choice place the trough askew. In spite of the statement of Pond, it may still be that some difference of opinion existed on the point in question, but as to this I cannot say. In the present Greenwich transit-circle no tilt of the kind can occur, since the trough is, in the direction of the meridian, a fixture.

Yours faithfully,

Blackheath, 1897, Oct. 14. WILLIAM ELLIS.

Star-Correction Tables.

GENTLEMEN,—

A copy of Prof. Turner's modified form of Mr. Stone's Tables for facilitating the computation of star-constants has just reached the Cape. I have examined it with much interest, as this is a subject to which I have given a considerable amount of attention. Like Prof. Turner, I have had a large experience of the time required for the computation of star-corrections, and it has long been my earnest endeavour to shorten it in some way.

Mr. Stone's Tables were a great advance in this direction, and their utility has been universally recognized; but still one always felt that too much time was spent on this item of meridian reductions.

Prof. Turner's modified form of these Tables will save some work, but not very much. The number of operations remains the same, though the number of figures to be dealt with is considerably reduced. The accuracy secured is ample for ordinary meridian work, but might be insufficient for refined researches. In Prof. Turner's example on page xi there is an instance of the accumulation of errors, due to contraction of figures all in one direction: the second of the two small Moon terms in R.A. is wrong by $0^{\rm s}{\cdot}08$ from this cause. Of course $0^{\rm s}{\cdot}1$ does not much matter in the case of a very close polar-star, but still why allow this uncertainty when it can be easily avoided? For comparison with Prof. Turner's example I will ask you to insert the following reduction by my Tables (which will shortly be in the hands of astronomers):—

1897 Dec. 1, λ Ursæ Minoris.. $\delta = +88^\circ\ 58'\ 54''{\cdot}3$.

The star will pass the meridian at Greenwich at about $3^{\rm h}$ M.T., and for this time we take the auxiliary quantities from the Independent Day-Numbers published by the Cape Observatory. The computation of the star-corrections is as follows (for notation &c. *cf.* Appendix to the Cape Meridian Observations, 1890–1891):—

$x = +{\cdot}213,\quad y = +{\cdot}095,\quad i = +2''{\cdot}82,\quad g'/g_0 = {\cdot}006.$

	h	m		h	m		h	m
α...	19	25·9	α...	19	25·9	α...	19	26
G...	23	20·7	H...	1	14·8	G'...	20	36
G+α...	18	46·6	H+α...	20	40·7	G'+α...	16	2

		s
P	1·309	
Px	+·279	
P(1+x)	....	−1·588
Q	0·942	
Qy	+·089	
Q(1+y)	....	−1·031
$g'/g_0\pi$	....	−0·007
log sum	....	−0·4194
log sec δ	....	1·7503
sum	....	−2·1697
n.n.	....	−147·81
f	....	+ 3·68
f'	....	+ 0·01
Correction	....	−144·12

	"	"
P'	4·05	
P'x	+·86	
P'(1+x)	....	+ 4·91
Q'	11·93	
Q'y	+ 1·13	
Q'(1+y)	....	+13·06
$g'/g_0\pi'$	....	− ·06
I	....	+ ·05
Correction	....	+17·96

If $-0^{s}\cdot71$ be applied to the R.A. reduction for terms of the second order, and $-0^{s}\cdot05$ for proper motion, the N.A. place for the star will be almost exactly reproduced. The discordance between Prof. Turner's result and mine in R.A. is chiefly due to an insufficient number of figures being used in his last two terms.

Prof. Turner's statement, on page viii of his Introduction, as to the effect of using Apparent places in the computation of the star-corrections instead of Mean places, is erroneous as regards R.A. There is a mistake of sign in the last term of his formula at the top of the page, and the result should read:

$$\Delta a'_0 \sec(\delta + \Delta\delta) = \Delta a_0 \sec\delta + 2\mu\Delta a_0 \Delta\delta_0 \sec^2\delta,$$

so that the R.A. is over-corrected in the same way as the Declination. If the mean of the Apparent and Mean places were used, there would be practically no need of further correction.

Royal Observatory,
Cape of Good Hope,
1897, Aug. 9.

Yours faithfully,
W. H. Finlay.

[I am glad Mr. Finlay considers that the accuracy secured by the Tables is "ample for ordinary meridian work," for this is all that is necessary. By "more refined researches" I suppose he does not mean differential work, for which quite different tables would be suitable, but work on absolute positions with instruments such as the zenith-tube or almucantar. With such instruments the probable error of an observation may be reduced to one-half or even to one-third of that for the transit-circle, but scarcely further; and the accuracy of the tables would still be quite sufficient.

As regards the accumulation of errors in the correction for λ Ursæ Minoris amounting to $0^{s}\cdot08$, this quantity is equivalent to $0^{s}\cdot002$ on the equator; and since the probable error of a meridian observation is greater than $0^{s}\cdot02$ for all observers, it is mere waste of time to correct it by quantities less than one-tenth of its probable error *when these are accidental and not systematic.* Now terms depending on the Moon's longitude may clearly be regarded as accidental errors. The phases of the Moon change often enough during the year to make them so. Personally I think the application of these terms a useless refinement altogether; it has certainly no sensible effect on star-catalogues, the ultimate outcome of meridian observations, and might just as well be omitted, as it was in time past. On the other hand, the quantity E given in the N.A., which changes slowly, is rightly retained, though the coefficient of the largest term (−0″·0451 sin ☊) is only half that of the largest longitude term (+0″·0895 cos 2☽).

I do not like to say more of what is practically only a repetition of the Introduction to the Tables, where I have tried to make it clear that we have been using too many figures in computations. But this habit is of long standing, and if there are good reasons for breaking it, perhaps a single statement is not sufficient:

perhaps the reasons should be urged again and again with an iteration comparable with the number of roots which the habit has struck.

Mr. Finlay is clearly reluctant to part with the long strings of figures; he likes to compute the corrections to 0^s·001 (equatorial), though the probable error of an observation is 30 times this amount at least. I think it is not only wasteful (I will concede him, if he likes, that the waste is not large) but unworkmanlike. It shows a mistrust of our tools to insist on using a fine one when we know that a coarser one is "amply sufficient."

To pass to Mr. Finlay's Tables, I sincerely congratulate him on having published them. If the saving of labour is what it seems from the example given, they should be very useful indeed; and though this is the first opportunity I have had of expressing this appreciation publicly, I have years ago said so to him. He described these Tables in M. N., vol. *l.* p. 497 (June 1890), and some time afterwards I wrote to him hoping that he would publish them; but I received no reply, probably because he could not give any definite information about the publication. I have not yet seen a copy of the tables, but I look forward to seeing them with great interest. It may very well be that they are an advance on Mr. Stone's tables—which (as is distinctly stated on the title-page) I have merely reprinted in more commodious form.

I am very much obliged to Mr. Finlay for calling attention to an error in sign, and very sorry for its occurrence. Three pairs of eyes looked over the proofs, but apparently the error escaped them all.—H. H. TURNER.]

Tuttle's Comets.

GENTLEMEN,—

When two or more literary or scientific men bearing the same name acquire reputation in the same branch, there is often some difficulty in distinguishing between the respective works of each. About forty years ago two gentlemen of the name of Tuttle made observations at the Harvard College Observatory. Of these we first hear of Charles W. Tuttle, who discovered the first comet of 1853 on March 8, but was anticipated by Secchi, who saw it on March 6. The discoveries of Comets I. and VII. 1858 are announced in Ast. Nach. vol. xlvii. col. 335 and xlviii. col. 287 respectively as having been made by Tuttle. That of the former (afterwards found to have been previously discovered by Méchain in 1790, and to have a period of 13·7 years) is also recorded in the 'Astronomical Journal,' vol. v. p. 101, by which we know that the discoverer was H. P. (Horace Parnell) Tuttle; but it remains uncertain whether that of the latter was due to him or to C. W. Tuttle. Elements of the orbit of the third comet of 1860 were published by H. P. Tuttle in Ast. Nach. vol. liv. col. 5; but this is erroneously ascribed to C. W. Tuttle in the 'Royal Society List of Scientific Papers.' ("The Owner of the Note-Book" speaks of the great value of this List, which would be much greater if it were more nearly up to date.) The discovery of Comet III. 1861 is announced in the 'Comptes Rendus,' vol. liv. p. (265), under the name "Tuttle" only. The last communication, so far as I am aware, from either of these astronomers is one containing elements of the third comet of 1862 (now known to move in the same orbit with the Perseid meteors), which was published by H. P. Tuttle in Ast. Nach. vol. lviii. col. 223.

Yours faithfully,
W. T. LYNN.

Blackheath, 1897, Sept. 15.

Jupiter's Satellites invisible, 1771.

GENTLEMEN,—

It seems somewhat strange that there is no record of Jupiter being observed apparently without satellites from 1681 to 1802, while there are three or four since the latter date.

On turning to the 'Nautical Almanac' for 1771 I find the configuration of Jupiter's satellites, at 6^h on the evening of November 2nd of that year, consists of a white circle for the 1st, and dark balls for 2, 3, and 4, indicating an apparent entire disappearance, the explanation given on p. 159 being: "The cypher signifies that the satellite is invisible on the Face of Jupiter; and the black mark signifies that it is invisible, being eclipsed in Jupiter's shadow, or behind Jupiter and eclipsed by his body." It would be interesting if some observation could be found.

Yours faithfully,
S. J. JOHNSON.

Melplash Vicarage,
Bridport, 1897, Sept. 13.

OBSERVATORIES.

MADRAS.—The first item that one looks for in Prof. Michie-Smith's Annual Reports nowadays is the record of the progress of the new observatory at Kodaikamal. We regret to see from the report we have recently received, which covers the period of twelve months ending 1897, March 31, that actual work on the building had not been begun by that date, although the greater part of the plans had been sanctioned and the money provided. Prof. Michie-Smith thinks that there is reason for hoping that the work will be begun soon, but is prepared for disappointment. Meanwhile the instruments for Kodaikamal are being collected at Madras.

The astronomical work at Madras during the twelve months has been first the necessary observations for time-determination. The observatory actuates a time-signal gun and a semaphore, and distributes a signal over all India and Burma at 4 P.M. The telegraph-lines in future for these purposes are to be in charge of the Telegraph Department. We wonder why India does not join in with the Universal Time System, and adopt a Standard Time depending on the Greenwich meridian, as most of the British colonies have done.

During the year the latitude of the Madras Observatory has been determined by Capt. Burrard, of the Great Trigonometrical Survey, with a zenith telescope and with a zenith sector. Prof. Michie-Smith also made a series of observations with the latter instrument which are not yet completed, but the result already deduced is $13^\circ\ 4'\ 8''\cdot 01 \pm 0''\cdot 08$.

The preparation of the Madras Catalogue has been continued, and it is anticipated that this will be published by the end of the current year.

The meteorological observations have been continued as before, and the reductions kept up to date.

THE following brief reports of the work done in Continental Observatories during the year 1896 have been extracted, as in former years, from the 'Vierteljahrschrift der Astronomische Gesellschaft':—

BAMBERG.—Heliometer measures have been made of the diameters of the Sun, Venus, Jupiter, and the distance of Mösting A from the Moon's limb. Comet Perrine was observed on six nights. 589 observations of variable stars were made on 51 nights. The first contact of the Moon with the Sun's disk was observed on 1896 Aug. 8, but clouds prevented any further observations. An attempt was made to obtain trails of the Perseid Meteors on a photographic plate, but without any result.

BÂLE.—Meteorological observations. The astronomical instruments were only used for demonstration purposes in connection with the University.

BERLIN.—With the transit-circle Dr. Battermann made 931 observations of transits, 595 of declination, 279 of level, 136 of azimuth, and 8 of collimation. The observation of Rümker's stars was finished. A screen on the object-glass was used for determination of personality in observing bright and faint stars. Progress is being made with the catalogue for 1895; the mean error of a single observation is found to be $\pm 0''\cdot 31$. A large number of occultations have been observed; about 300 disappearances and 200 reappearances in 2½ years. With the 9-inch refractor Prof. Knorre made 352 determinations of distance, and 400 of position-angle of double-stars with a double-image micrometer. The stars chosen were from those which appear to show no relative motion. Prof. Goldstein has been engaged on a work on kathode rays with a view to their furnishing an explanation of "kosmischer Lichterscheinungen." Photographic observations of latitude have been made with the zenith telescope, 195 determinations of latitude and 85 of scale value being obtained. The time-service has been carried on satisfactorily.

Berlin. *Die Astronomische Recheninstitut.*—To the usual work for the preparation of the 'Jahrbuch' was added a considerable amount of new computations, in view of the changes to be introduced in the Ephemerides for 1901. Preparatory work for the formation of Ephemerides of the Major planets from Hill and Newcomb's tables has been carried out. Preparations for the introduction of the new fundamental catalogue of Dr. Auwers, have also been made. As usual a good deal of attention has been bestowed on small planets. The ephemerides of 62 of these have been computed for the times of opposition. Dr. Ginzel has made extensive investigations into the conditions of the eclipses which occurred between 900 B.C. and 600 A.D., visible between 10° W. and 60° E. long., and latitudes 25° and 50°. For 485 central and partial solar eclipses the magnitude of the phase and the hour-angle are given for Rome, Athens, Memphis, and Babylon. Similar data are given for 1627 eclipses of the Moon during this period. He also discusses 81 eclipses mentioned by Roman, Greek, Byzantine, and Frankish writers and chroniclers of the Middle Ages, the 19 eclipses of the Moon mentioned in Ptolemy's 'Almagest,' and a number of eclipses referred to in Assyrian and Babylonian records. The object of the work is twofold, as it bears on certain historical questions and also on the Moon's motion.

Bonn.—With the 6-inch Repsold transit-circle 4829 observations of stars have been made towards a Catalogue for the year 1900. For variation of personal equation with magnitude experiments have been made with screens on the object-glass. Prof. Deichmüller is proceeding with a comparison of the Bonn Zone Observations with those of earlier catalogues. He has also been engaged on a new method of determining zenith-points, which is described in Ast. Nach. 3394. The 4-inch Ertel Transit-instrument has been used in the Prime-vertical for determination of latitude.

[To be continued.]

PUBLICATIONS.

Recent and Coming Eclipses*.—In his recently published book with this title Sir Norman Lockyer tells us that he has had experience of six solar eclipses; this, apart from his well-known skill as an author, might be considered sufficient to constitute him the fittest person to write a book on the subject, and the present, in view of the approaching Indian eclipse of 1898, is obviously a fitting time for its production. The work, a great part of which we believe has appeared already in 'Nature,' is, as may be judged from its title, a history of the eclipse of 1896 and an account of the plans and prospects for that of January 1898, and this necessarily involves some description of spectroscopic methods and results. After a short introduction on the geometry of eclipses, the second chapter gives instructions as to the work that may be done by observers without any elaborate equipment, which is followed by chapters of a more advanced kind, with a description of Sir Norman Lockyer's particular instrument, the prismatic camera, thus putting the reader in possession of the latest facts relating to spectroscopic observation. Then follows the history of the eclipse of 1896, so far, that is, as the author's expedition to Norway was concerned, and so far as Prof. Lockyer was concerned with the successful Nova Zemlya expedition; but it may be noted that the expeditions to Japan are not mentioned in the book, nor is the name of the late Mr. Stone. However, the chapters describing the expedition to Kio Island are especially interesting to read, and valuable in showing how it is possible to convert a ship's crew such as that of H.M.S. 'Volage' into a huge band of astronomical observers, which, as Prof. Lockyer said, in a stately interchange of compliments with Capt. King Hall, was no mean result of the expedition. Sir Norman Lockyer then goes on to speak of Mr. Shackleton's successful results at Nova Zemlya; he thinks that the spectrum of the flash so happily secured, as well as the photographs of 1893, support his view as to the non-existence of the "reversing layer," and that both photographs show simply the spectrum of the chromosphere. Also considering the photographs of spectra obtained in the eclipses of 1893 and 1896 with respect to his recent investigations of the enhancement of lines on the spectra of "hot" stars, the author deduces that the chromosphere is a region of high temperature in which there is a corresponding simplification of spectrum as compared with the cooler region in which the Fraunhofer system is produced. The difference of the shape of the corona as observed at different epochs of the sun-spot period is touched on, but coronal photography is not treated in such detail as spectroscopy. A chapter on the weather prospects and the locality of the line of totality, with plans of work for next January, closes a very opportune and instructive book.

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* 'Recent and Coming Eclipses,' by Sir Norman Lockyer, K.C.B., F.R.S. London: Macmillan and Co., 1897. 6s.

MEMORIALS OF THE BOND FAMILY.—In these days, when America contributes so much to Astronomy, it is hard to realize that the science was scarcely followed at all on the other side of the Atlantic at the beginning of this century, and that the Harvard College Observatory, now the most flourishing of institutions, is barely sixty years old. From Prof. Holden's most recent work * the reader may learn of the beginning and early growth of American astronomy, and how far the Bonds, father and son, were associated with this.

* 'Memorials of William Cranch Bond, and of his Son, George Phillips Bond,' by Edward S. Holden. Murdock and Co., San Francisco; Lemcke and Buechner, New York, 1897.

The subjects of this book claim descent from an old Cornish family, the Bonds of Brendon, who have an undoubted ancestral record dating from Norman times. William Bond, father of William Cranch Bond, emigrated to America in 1786, and originated a shipping business; but this not being successful, he established a firm of watch and clock makers, which still flourishes under the original name. His son, William Cranch Bond, afterwards the first director of the Harvard College Observatory, was brought up to this business, and at an early age proved himself a capable workman. He devoted his leisure to astronomy, and his first success appears to have been the discovery of the comet of 1811 a few weeks after its discovery in Europe. He must have gained considerable public reputation as an astronomer, for when, in 1815, a committee of officials of Harvard University was appointed to consider the establishment of an observatory, W. C. Bond, then twenty-six years of age, who was going to Europe, was entrusted with the mission of enquiring into the construction and methods of European observatories. His account of the mission is printed in vol. i. of the Harvard Annals.

But though a Harvard College Observatory was talked of in 1815, its establishment was not accomplished as an actual fact until many years later. Prof. Loomis is responsible for the statement that the return of Halley's Comet in 1835–6 caused a wave of astronomical enthusiasm to pass over the continent which led to the establishment of several American observatories. In 1838 William Cranch Bond was appointed Astronomical Observer to Harvard University, a position valuable only in name, for the office had no salary attached to it until 1846, the only emolument being the use of the Dana House as residence, and besides the acceptance of the post necessitated Mr. Bond's separation from his business.

The new observatory—the Harvard College Observatory of to-day—begun in 1843, was practically finished in 1847, and the great equatorial was set up by July 24 of that year. The work of the Bonds with that instrument is described in the book; it may be sufficient to merely mention here their observations of the nebulæ of Orion and Andromeda, the observation of Saturn, the discovery of the crape ring and of Hyperion, the measures of the satellites of Neptune, and the observations of the comet of 1858; but possibly the most valuable contributions of W. C. Bond to astronomy are, first, the invention of the chronograph, the credit for which has been given elsewhere, but there seems no doubt but that the Bond family were the first to construct an actual instrument of this kind and to apply it to astronomical work; and, secondly, the inception of the methods of astronomical photography.

William Bond died in 1859, and his third son, George Phillips Bond, succeeded him as Director of the Observatory. William Bond's eldest son had died young, and the two others continued the chronometer business. The emoluments of the Director although to them was added the salary of the Phillips Professor of Astronomy, were still inadequate for the post, and we have it on the word of his daughter, that the life of George Bond was one of some privation. The section of the book devoted to him is made interesting to us by extracts from his diaries during visits to Europe in 1851 and 1863; it will be noticed that the earlier date is the year of the Great Exhibition, where the Bond chronograph was exhibited and received a Council award. We should have liked, if space allowed, to quote from these diaries, which form the record of extended visits to the observatories of Europe, as they furnish vivid pictures of men now known to us only by name and of scenes and functions at which the younger Bond was present. Writing of the Greenwich Visitation Dinner, he says:—"I noticed that the most distinguished scientific men are bad hesitating speakers, except, perhaps, Sir John Herschel. Doctor Lloyd, Mr. Airy, Mr. Adams, &c. are instances." We give this merely as illustrative of about 60 pages of the book, which will afford any astronomer interesting reading.

There is little for us to add as to the work of George Bond; besides the direction of the Observatory, he worked much at cometary physics and at the theory of astronomical photography. This is all recorded in a chapter of Prof. Holden's book in which his correspondence with other astronomers is collected. We refrain from saying anything as to the literary style of the book, for this is quite secondary in a work of this kind, which is mainly a collection of memoranda, diaries, and letters; but Prof. Holden is to be congratulated on the compilation of an interesting book which should find a place in every astronomical library.

UNTERSUCHUNGEN ÜBER DIE GRÖSSE UND HELLIGKEIT DER KOMETEN UND IHRER SCHWEIFE. Von Dr. JOHANN HOLETSCHEK. (Vienna, 1896.)—We have recently seen the first volume of thi work, which deals with comets up to the year 1760. The author has collected all estimates of the magnitude or brightness of comets, and reduced them to $r=1$, $\Delta=1$, and hence has been able to classify them in regard to their intrinsic brightness. It is scarcely necessary to remark that there is much uncertainty and personality in such observations, especially in pre-telescopic days. Thus Tycho Brahé considered that the fixed stars had real diameters, and he appears to have classified comets by the apparent diameter of the nucleus rather than by its brightness. Observations of the day on which a comet becomes invisible to the naked eye are valuable, as there is considerably less uncertainty in determining this than in making an estimate of the stellar magnitude of a nebulous object. There is a discussion of the allowance to be made for atmospheric absorption, and of the law according to which the brightness changes with varying distance from the Sun.

Then follows a detailed discussion of the intrinsic brightness and true diameter of coma and length of tail of all the comets on record up to the year 1760 for which orbits have been computed.

The case of the different apparitions of Halley's Comet is considered in special detail, and the results show a very satisfactory agreement, the reduced magnitude of the comet at most apparitions being about $4^{m} \cdot 0$.

This is certainly a very useful work of reference, and the author has done his best to treat with scientific method a subject that does not readily lend itself to accurate discussion. A. C. D. C.

NOTES.

Comet Notes.—A bright telescopic comet, with a nucleus and short tail, was discovered by Mr. Perrine at the Lick Observatory on Oct. 16. From its high north declination it is well placed for observation throughout the night, and numerous observations have been obtained at Greenwich and elsewhere.

The following elements have been deduced by Profs. Kreutz and Möller from observations made on Oct. 16, 18, 20:—

$T = 1897$ Dec. 7·80 Berlin M.T.

ω	65° 4′	1897·0.
☊	31 58	
i	69 27	
$\log q$......	0·1344	

The following ephemeris (for Berlin midnight) has been deduced from these elements:—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Nov. 1....	21 26 4	80 52	Nov. 12....	18 48 40	70 14
2....	20 55 44	80 10	14....	18 40 0	68 17
3....	20 30 21	79 20	16....	18 33 24	66 27
4....	20 9 25	78 24	18....	18 28 12	64 42
5....	19 52 8	77 25	20....	18 24 0	63 3
6....	19 37 51	76 24	22....	18 20 40	61 34
8....	19 16 20	74 18	26....	18 15 44	58 45
10....	19 0 36	72 15	30....	18 12 24	56 21

The brightness on Nov. 1 is 1·1 times that at discovery, and on Nov. 30 0·7 times that at discovery.

J. Bauschinger gives, in A. N. 3453, the following ephemeris of Brooks' Comet 1896 VI. and 1889 V., which will probably be again visible with powerful instruments during the coming winter:—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Dec. 17....	8 32 54	26 59	Jan. 10....	8 14 24	28 17
25....	8 27 40	27 26	18....	8 7 7	28 36
Jan. 2....	8 21 23	27 53	26....	7 59 57	28 51

Ast. Nach. 3447 contains a search ephemeris for the approaching return of Winnecke's Comet, by C. Hildebrand.

This comet approached Jupiter within a distance of 0·5 in December 1894, and then suffered large perturbations, which have been investigated. The approaching return is an unfavourable one, and the comet is not likely to be visible before the middle of December. It is brightest at the end of February next, and passes perihelion about March 20.

Ephemeris for Berlin Midnight.

	R.A.			N. Dec.	
	h	m	s	°	′
Nov. 3....	12	33	17	8	59
7....	12	42	8	8	13
11....	12	51	12	7	27
15....	13	0	30	6	39
Nov. 19....	13	10	3	5	51
23....	13	19	53	5	2
27....	13	30	0	4	12
Dec. 1....	13	40	26	3	21

A. C. D. C.

THE DETERMINATION OF STAR-PLACES BY PHOTOGRAPHY.—Another method for adapting the photographic plate to the transit-circle is before us. This is by Mr. Lippmann, the re-inventor of the coelostat, who describes a scheme for this in the 'Bulletin Astronomique' for October. He does not call his apparatus a Photographic Transit-circle, but in its main principles it is not unlike the instrument of this name now being made by Prof. Turner, and referred to by him in the M. N. R. A. S. for March last. The essential features of both plans are that the plate and the stars shall be relatively at rest, but that both shall move relatively to a line or spot which defines the meridian. In both cases the photographic telescope is fixed. In the Oxford instrument the stars are reflected into this by a plane mirror rotating on an axis transverse to the meridian, which practically corresponds to the axis of a visual transit-circle, and the plate in the telescope is made to follow the stars as they move. M. Lippmann reflects the star-images into the photographic telescope by means of a coelostat, and they are by this means at rest relatively to the plate. Prof. Turner makes fiducial marks on the plate by a spot of light which, controlled by the standard clock, shines every second. M. Lippmann photographs on his plate, also at definite instants, an image of a diaphragm which is made to represent the meridian, and a short line at right angles to this. Thus, when M. Lippmann has developed his plate he finds, besides the stars, a reticule, the lines of which represent hour-circles and circles of declination.

Definitely, but briefly, M. Lippmann's apparatus consists of: (1) a coelostat; (2) a fixed photographic telescope with fixed plate, adjusted to work with the coelostat; (3) a collimator at right angles to the meridian, carrying a diaphragm, with means of lighting it, in the place of the eyepiece, and a totally reflecting prism at some distance in front of the object-glass. The collimator telescope is placed so that the prism is above the coelostat, and the image of the diaphragm (lines cut on silvered glass) is reflected by the prism on to the coelostat mirror, and thence into the photographic telescope. The collimator, M. Lippmann says, is the only part of the apparatus which requires adjustment, for this has to be placed so that the diaphragm line corresponds to the meridian, and its errors have to be determined by methods analogous to those adopted for the ordinary visual transit-circle.

The above description may suffice to give an idea of the apparatus; copious details will be found in the original memoir. But ingeniously devised though it may be, we cannot follow the author when he claims for its use, as he does, a superiority over the methods being used for the formation of the Astrographic Catalogue. He impeaches this latter work because it has to depend on a catalogue of fundamental stars made by visual observation, which is therefore affected by personal error, and, secondly, because the construction of the catalogue will take twelve or fifteen years. But M. Lippmann will no doubt see that any catalogue, photographic or otherwise, must depend on a fundamental catalogue of clock-stars; and beginning afresh with his photographic instrument to make a clock-star list, his catalogue proper would scarcely be begun within the limit of time he has set down for the astrographic work. It is not impossible, and not a very immense work, to correct the places of the fundamental stars by the photographic plates themselves, and so eliminate personality. In M. Lippmann's scheme the plates will have to be measured and corrections applied to these measures to find equatorial coordinates, just as in the work now going on similar processes give rectangular coordinates, but we are of opinion that in the latter case the application of corrections can be done more rapidly, and hence we fail to see where would be the saving of time in the proposed method, whilst the complication of M. Lippmann's telescopic arrangements, which involve, besides the standard clock, a clock to drive the coelostat, the accurate going of which appears to be essential, seems to be likely to increase the time spent in taking the photographs. H. P. H.

OCCULTATION OF CERES.—Col. G. L. Tupman kindly informs us that there will be an occultation of the planet Ceres by the Moon on Nov. 13. The following prediction is computed by him for Greenwich:—

	G. M. T.		P.	v.
	h	m	°	°
Disappearance	10	36	54	97
Reappearance	11	30	315	357

Ceres will rise at $7^h\ 0^m$.

RESIGNATION OF THE DIRECTOR OF THE LICK OBSERVATORY.—After a continuous connection with the Lick Observatory for 23 years, and a service at Mount Hamilton since 1888, Prof. E. S.

Holden has terminated his official relations with the Observatory. The resignation is to take effect on December 31, 1897. Prof. Holden's address after October 1 will be "Smithsonian Institution, Washington, D.C., U.S.A."

We have seen announced the establishment of a new observatory at Valkenburg, Holland, attached to the Jesuit College of that city. The equipment consists mainly of an equatorial of nine inches aperture, constructed by Mr. Saegmuller, and the optical part by Mr. Clacey. This instrument and its accessories have been designed chiefly with the idea of observing variable stars, which is the branch to which the first Director, Rev. J. Hagem, S.J., has hitherto devoted himself.

The opening boom of the Yerkes Observatory has been heard in England. On October 20 most of the newspapers published a telegram from across the Atlantic, reporting that Prof. Barnard had observed the Moon with the 40-inch, and had said that he saw an enormous number of details never seen before, but there were no traces of water, air, or life in any form to be detected, and he went on to say that the observations made failed to give any confirmation of the theories of the Paris astronomers. Let us assure Prof. Barnard that we do not believe that he could have been so dogmatic.

It is announced that Baron d'Englehardt, of Dresden, an energetic amateur astronomer, is giving up astronomical work, owing to infirmity caused by advancing years. He has presented his instruments and library to the Russian Imperial Observatory of Kasan. We regret to think that we shall receive no more of the Baron d'Englehardt's handsome publications, the last of which was referred to in our number of 1896, April.

The post of Astronomer Royal for Ireland and Director of the Dunsink Observatory is still unfilled. The authorities of Trinity College, Dublin, are advertising that intending candidates should send in their names to the Registrar by Nov. 10.

Prof. Barnard has secured another photograph of a meteor. At the time of the last Perseid shower he exposed plates on the mornings of August 10, 11, and 12, after moonset. Two cameras were used, a 1½-inch lantern-lens of 6·4-inch focus, and a $3\frac{4}{10}$-inch Clark portrait-lens of 19·4-inch focus. On both of these a meteor left its trail on the morning of August 10.

The series of articles on the Meteor Shower of November which have been appearing in these pages between March and October have been republished in one cover by Messrs. Taylor and Francis. The published price is one shilling.

We hear that Mr. Arthur Mee has published an enlarged and much improved edition of his 'Observational Astronomy.'

FROM AN OXFORD NOTE-BOOK.

DURING the past year we have lost by death a large number of astronomers of some note. One whose loss will be widely felt, though his position in life was a comparatively humble one, was Albert Marth, who died in Germany a few weeks ago. I do not know the circumstances of his death, but as he was passing through London to Germany I met him at the R. A. S. rooms, and he told me he was going for his health's sake. He seemed as vivacious and energetic as ever, and, finding a sympathetic listener, talked away at a great rate. The conversation ran on mistakes and misprints, and I find in the Note-Book the following notes of small errors to which he drew attention, and which are perhaps sufficiently characteristic of the man to reproduce here:—

(1) 'American Ephemeris Papers,' vol. vi. part 1, Table III. in Newcomb's Tables of the Sun:—Sept. 4 is followed by Sept. 19, and the whole date column is wrong from this point. (Those in possession of these tables would do well to correct this at once, as it may easily lead to error in using them.)

(2) Ibid., vol. vii. part 2, p. 194, line 10:—For 1801 to 1900 read 1800 to 1899. Similarly in part 1, p. 57, line 28.

(3) In Introduction to B. A. C. p. 28:—"I have already observed that, in these tables, the year is supposed to commence on January 1st at that moment of time when the Sun's mean longitude *at mean noon* at Greenwich is exactly 281°." *Dele* the words in italics, which are curiously superfluous.

THE following small items of news may not have found their way through more ordinary channels:—Mr. Charles Burckhalter will be in India for the eclipse, and will, I hope, this time see what his buzzing exposure shutter can do. On the other hand, it seems only too likely that Dr. Common will *not* be able to go to India. From the Lick Observatory comes the intelligence that Director E. S. Holden has resigned, and Professor Schaeberle has been appointed Acting-Director.

A PARAGRAPH written by Mr. James Payn in another "Note-Book" * would be spoilt by any alteration, and so I venture to transcribe the following paragraph *verbatim*, though it is only the end of it which has a legitimate place here. I have only altered the spelling "Airey" to "Airy":—

> A good book has recently been published concerning epitaphs and epigrams. Though generally found together they are very different matters. Some lines which will probably not be in the book run in my memory, but from what source I have no idea :
>
> "Of all my mother's children
> I love myself the best;
> As long as I'm provided for
> The devil take—the rest."
>
> That can never have been put over a grave, at all events by the occupier. It is too frank, and, it must be confessed, ungenial. Can anyone give them a

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* "Our Note-Book" in the 'Illustrated London News' for 1897 October 16.

local habitation? The best epigram, by-the-bye, or rather the best application of a quotation in our time, was made on Professor Airy, who was made Plumian Professor at Cambridge and given the Observatory to live in by the University, but without income. "They gave to Airy nothing, a local habitation and a name."

Now one or two of my stories, told in these pages, have been questioned lately, and one of them even flatly contradicted, not to say disproved. In fact I may admit that the bottom was knocked right out of it. So it is only human nature to see if one can find any flaw in other people's stories, and I promptly took down Airy's autobiography to see what I could find bearing upon the subject. It would perhaps be too much to expect to find the actual story there—I wonder whether he ever heard it!—but there is enough to give it a good firm foundation. "I had made known that I was a candidate for the Plumian Professorship," Airy writes of events in 1828, "and nobody thought it worth while to oppose me. . . . But meantime I told everybody that the salary (about £300) was not sufficient for me, and on Jan. 20th I drafted a manifesto or application to the University for an increase of salary." But although a syndicate was ultimately appointed to consider the matter, it was more than a year before they could do anything.

> The Observatory house was furnished partly from Woodhouse's sale and partly from new furniture. . . . On March 27th, 1828, I slept at the Observatory for the first time, and on April 15th I came to reside there permanently, and gave up my College rooms.

Professor Falb, of Austria, and Professor S. J. Corrigan, of Minnesota, both seem to be trying to get up a little excitement by announcing the end of the world. The former gives the date when time shall end as 3.3 P.M. on November 13, 1899. [I am quoting an evening paper; I hope it has not altered the hour, consciously or unconsciously, to suit its own interests.] The latter (still quoting the evening paper, which was thoughtfully sent to me) makes the "disclosure" that the "sun-spots are due to the evolution from the Sun of an entirely new planet" which "may at any instant break away from the Sun, and the terrific explosion which will necessarily accompany this breaking away will produce a great disturbance of the entire universe, but particularly of the Earth, perhaps completely smashing it, and surely destroying all animal life on land as well as in the waters." With truly noble self-depreciation, Professor Corrigan refuses to claim complete originality for his idea. "Neither is this tremendous disturbance of the Earth and the destruction of all life upon it completely unprecedented," he admits; for there was a similar little accident just 23,000,000 years ago, as is well known to scientists. Had it not been for this, it might never have occurred to Professor Corrigan to examine the probabilities of the Sun being in labour, and we might never have had this most interesting announcement—I beg pardon, "disclosure." But now the least he can do is to supply us with regular bulletins on the condition of his interesting patient. We are naturally all deeply concerned, even "scandal about Queen Elizabeth" would not be "in it" with "disclosures" concerning the Sun.

Which reminds me that a grave accusation was made against the Sun during the month of August. A Dartford publican, who had been summoned for having sold whisky and rum below the legal standard of strength, pleaded that the Sun's rays had acted upon the stoppers of the glass and porcelain vessels containing the spirits, causing great evaporation and reducing the strength. Surely the fine of 10*s.* and costs imposed is hopelessly inadequate? Where are Mr. Labouchere and the pillory? Another correspondent kindly sent me the following clipping from a local paper of September 1. This wonderful bargain must have long ago been snapped up, so that the Editor of this Magazine will not be able to claim advertisement rates for the insertion:—

A Valuable Astronomical Telescope, 9 feet long, with brass adjustment and finder attached. Object glasses, 8 inches; coloured glasses, shades and tubes for day and night uses in mahogany box complete; mahogany stand with sliding and screw adjustments; Price £15.– Box 491, *East Anglican Daily Times*, Ipswich.

This note-book, usually so serious, seems this month to have produced a number of paragraphs of gradually increasing frivolity, and it now wishes to wind up with some poetry. Mr. Ellis's interesting "Reminiscences" remind it of a "poem, or species of poem," which it picked up after one of the dinners on Greenwich Visitation Day; but we can only allow a small portion to appear:—

Since I was young and called to the stars,
  I've seen a number of changes;
'Tis nearly thirty years of Mars
  Over which my memory ranges.
The Visitors then didn't drink French pop
  In a salon of high-class finish;
They dined at four, on beer and a chop,
  At the 'Crown and Sceptre' in Greenwich.

In Airy's early days, I ween,
  Assistants were not many;
Computers few and far between,
  In fact there were scarcely any.
And so they had to work the more,
  For regular T. C. duty
Meant all the hours of the twenty-four,
  Which wore out youth and beauty.

They used to measure our distance from the Sun
  By watching Transits of Venus;
Now they choose from the planets one
  That doesn't even come between us.
And Dr. Gill declares he's able,
  By using a heliometer,
To beat the 7-figure logarithm table,
  Or any known arithmometer.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 260. DECEMBER. 1897.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, November 12, 1897.

Sir R. S. Ball, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., F.R.S., and H. F. Newall, M.A.

The Minutes of the previous Meeting were read and confirmed.

The President. Since the Society last met, Professor Turner and I discharged the duty which was entrusted to us of conveying the address adopted by the last Meeting to Her Majesty the Queen. We were received by the Prince of Wales with many other deputations which were presenting addresses, and the address of the Society was presented by us to His Royal Highness. I will read you the reply which we have received:—

> On behalf of the Queen, My dear Mother, I thank you for your loyal and dutiful Address, and for the affectionate Congratulations which you tender on the completion of the 60th year of Her Reign.
>
> It is a source of profound joy to The Queen to receive the expressions of devotion to Her Person and Family, which are offered by Her subjects throughout the Empire. She is gladdened by the thought that the 60 years of Her Reign have been years of progress in knowledge and of increase in prosperity; And She prays that, by the blessing of Almighty God, She may always live in the hearts of Her loving and beloved People.

I might also mention that in connection with this address presented to the Queen on the occasion of the Jubilee celebration, and growing out of it, the following letter came to me a few days ago. It was addressed to me as President of this Society, and therefore it is in that capacity that I now read it:—

> Lieut.-Col. Sir Fleetwood Edwards presents his compliments to Sir Robert Ball, and is commanded by the Queen to forward to him the accompanying Medal, to be worn in commemoration of the 60th anniversary of Her Majesty's Reign.

This is the Medal which Her Majesty has been pleased to confer on the President of the Royal Astronomical Society, as well as, I understand, on the Presidents of other learned Societies of this country.

Mr. Newall. The number of presents received since the date of the last Meeting amount to 194. I will call attention to one or two. Prof. Newcomb has sent us a copy of his new determination of the Constant of Precession; Prof. Holden, of the Lick Observatory, has forwarded to us a copy of his new work, 'Memorials of W. C. Bond and G. P. Bond'; we have received from the Adams Memorial Committee a copy of the first volume of the collected papers of the late Professor J. C. Adams; and we have also a very interesting volume from the Harvard College Observatory. Among such a number of presents it is difficult to select any for mention without being invidious, but I think I may add to what I have said that we have also received from Prof. Auwers a copy of his 'Catalogue of Fundamental Southern Stars,' and also several beautiful photographs, among them being one by Professor Barnard, of a meteor trail photographed on a plate of the Pleiades, and one by Mr. W. E. Wilson of the nebula in the constellation of Orion. From the Lick Observatory we have a photograph of the Moon and Jupiter. Dr. Palisa has sent us some star maps for the Equinox 1875, and we have also received vol. i. of the publications of the Zurich Observatory, which contains observations of sun-spots in the years 1887 to 1889.

Prof. Turner said that among the papers received that evening there were very few suitable for reading to the Society. There was one short paper by *Mr. John Tebbutt*, of Windsor, New South Wales, on "Equatorial Comparisons of Uranus with 41 Libræ, and a Probable Occultation of the Star by the Planet." The observations were made under fairly good conditions, and the conclusion drawn was that the planet must almost certainly have occulted the star. The occultation of any star of sensible brightness by any planet was a very rare phenomenon, and Mr. Tebbutt, in concluding the paper, suggested the desirability of improving the table of phenomena in the 'Nautical Almanac.'

Dr. Johnstone Stoney (presenting a paper "On the Approximate Ephemeris of the Leonids from 1897 December 24 to 1898 April 8"). This ephemeris is a continuation of the ephemeris in the 'Monthly Notices' of last December. This is an ephemeris of that part of the Leonid-swarm which passed close to the Earth in 1866, and when an attempt was made to photograph meteors in the open sky. It has been thought desirable that these observations should be repeated, since the conditions will be somewhat more favourable in the current season on account of the increasing intensity of sunshine upon the meteors. This will make the image of the swarm on the photographic plate about three times brighter than it was last season. Even so the chance of success seems but slight; but as the gain to astronomy, if successful photographs are obtained, will be great, it is desirable that preparations should be made for another attack. This ephemeris has been drawn up under the direction of Dr. Downing, Superintendent of the 'Nautical Almanac,' and the computations have been made by Mr. Thomas Wright of that Office. The cost has been defrayed from a grant made by the Royal Society.

Dr. Downing. I am pleased to have been able to take part in the preparation of this ephemeris, and I may say that the conditions are considerably more favourable this season than they were last. Last season the meteor-swarm was at about the distance of Saturn—this season it is at about the distance of Jupiter. I think Dr. Stoney is to be congratulated upon having secured the co-operation of Dr. Isaac Roberts; for if any man can photograph this swarm in the open sky, I am sure Dr. Roberts is the man.

Capt. Steele. I should like to ask what is the nearest approach of the track of the meteor-stream to the Earth at any time, and how long it would require a meteor to traverse that distance from its range to the Earth. I ask this because in 38 years I have seen a great number of meteors, and I do not recollect one that occupied more than 30 seconds of time in traversing its path. Also I cannot account for the fact that meteors beyond our atmosphere are devoid of illumination.

Dr. Stoney. I think the proper answer to that question is that one of the greatest investigations made by Prof. Couch Adams was of the orbit of these meteors; and it is from the elements of the orbit as determined by him that the present ephemeris has been constructed. The orbit is one which carries the meteors a little beyond the distance of Uranus. A portion of the thickest part of the swarm passed close to the Earth in 1866, and that same part of the swarm is now drawing close to the Sun and is at about the distance of Jupiter, and its position in the sky can be determined from the elements of the orbit as obtained from the remarkable investigation made by Prof. Adams. Of course the chance of the swarm giving sufficient light while in the open sky, even as it draws closer to the Sun, to impress a photographic plate is very slight; but in a case of this kind it seems to be almost what I may call an astronomical duty to attempt to photograph it, even though we are quite aware that the prospect of success is but small.

The President. There is one thing we shall all agree with, and that is we are very much obliged to Dr. Stoney for the trouble he has taken in preparing this ephemeris, and we must join in expressing thanks to Dr. Roberts and Dr. Downing for assisting him; and considering the great interest which attaches to this recurring swarm of meteors now within a year or two of the time when we ought to expect a return of the great group, it may be of interest if I read a letter which I have received from Professor Ridgway, of Cambridge. He writes as follows:—

My dear Sir Robert,—As the 14th is close at hand with its Leonids, it has occurred to me that perhaps it was to one of the grand displays that Shakespeare refers in 'A Midsummer Night's Dream,' Act. II. Sc. i. (Oberon's speech), "and certain stars shot madly from their spheres." The play was first printed in 1600, and the commentators all point out the references it contains to remarkable meteorological occurrences. The date would fit one of the big displays. I ought to say that 'A Midsummer Night's Dream' was known in 1598; but it would not at all follow that it contained the lines to which I refer, as Shakespeare had a way of gradually working up the plays. I daresay you know all about the passage, so forgive me for troubling you with this.

(Mr. Newall here left the Secretary's Chair, which was unoccupied for the rest of the evening.)

The President. Mr. Newall's leaving is not altogether without significance. He is this evening starting for Brindisi, and from there he will proceed to India to observe the Eclipse. He especially entreated me that I would make no reference to this. I promised him I would not. (Laughter.) Prof. Turner, however, considers that the promise was only binding so long as Mr. Newall was here, and that now I am absolved from it. (Laughter.) I am sure we all wish him an extremely happy time and a good view of the Eclipse. Those of us who went to Norway and came back with results not of a very high astronomical value will sympathize all the more with the success which we hope will be obtained by this expedition. Several other astronomers are going, but they are going in ones and twos at different times and by different routes, and Mr. Newall appears to be in the van with the party of observers who are going to conduct these observations in far distant India. We have heard various accounts of the dangers which may be anticipated. I am not sure of the number of thousands of snakes per week that the observers are likely to encounter, nor do I know whether it is true, as I am told, that the particular amusement in India in the evening is listening to the tigers sharpening their claws outside the tent. (Laughter.)

Photographs of meteors taken by *Prof. Barnard* at the Yerkes Observatory were here thrown upon the screen.

Dr. Stoney. I think it is of great interest and importance to find that photographs of meteors can be taken so successfully. One of Prof. Barnard's best photographs, he told me, of the nebula in the Pleiades, was taken in strong moonshine. There is an impression that it is only on extremely dark nights, and in the absence of moonshine, that photographs can be taken.

Prof. Turner. I should like to put on record an expression of regret, when we are talking about meteors, that we never have the pleasure of seeing amongst us our first British meteor observer, Mr. Denning. It would give us very great pleasure if we could have Mr. Denning amongst us, even for once, to hear what he has to say about the approaching display. Many of us are preparing to make observations of meteors during the next few years, though we may not have done so previously. Mr. Denning observes meteors not only in sensational years such as 1898 and 1899, but all the time; and he has recently published a little pamphlet on the subject, which is of the greatest interest. Many of us will make observations in the next few years who are quite new to the subject, and I only hope that his words of instruction will fall as seed on the good ground, and that we shall pay attention to the advice of an experienced observer. Mr. Denning does not place much faith in the photographic method. Perhaps that is natural in one who has spent so many hours in observing meteors by the eye. Photographs of meteors are very few in number, and the possibility of obtaining success by the photographic method must not prejudice eye-observers; and I would suggest that observers should follow the advice given by such an experienced student of meteors as Mr. Denning.

Dr. Stoney. I think it is very important that as the Moon will be shining brightly on the occasion of this year's display, it should be known widely that it is not hopeless to attempt to obtain good photographs.

Capt. Noble. I should like to say with regard to what Prof. Turner has said about Mr. Denning's book, that if anybody is beginning to study meteorites, he had better lay out a shilling on the book. It is an epitome of everything we know on the subject, and it is certainly a most admirable little pamphlet.

Mr. Chambers. I should like to say a word in partial reply to what Prof. Turner has said. I happened to have a few spare hours at Bristol some time ago, and I called upon Mr. Denning and had a very profitable interview with him. I came away with the impression that Mr. Denning, although a great genius in the matter of meteors, is not one of those self-advertising geniuses of whom we hear too much in the present day.

Dr. Stoney. Might I add that if the meteor-stream is following one definite course, its radiant-point as seen from the Earth will be slightly changed in consequence of the attraction of the Earth; hence in all cases the time of the observation should be recorded in order that the effect due to the attraction of the Earth may be subsequently computed.

Dr. W. J. S. Lockyer. I should like it to be known that Mr. Butler, of the Royal College of Science, about a year and a half ago, took a photograph of a magnificent meteor, which burst right in front of the camera. He put the camera outside his bedroom window to test it, and gave half an hour's exposure, and it happened that a meteor burst in front of the camera; a very good photograph was the result.

Prof. Turner said that the Society possessed this photograph, and it would be put upon the screen. (Photograph thrown on screen.)

Dr. Spitta. Can Dr. Lockyer give us any information about the lens?

Dr. Lockyer. I think it was about 1 inch; it was one of the object-glasses of a field-glass, and the focus would be 7 or 8 inches.

Mr. Shackleton described his paper on "A Spectroscopic Method of determining the exact instant of the Second and Third Contacts during a Total Eclipse of the Sun." After referring to the spectroscopic method devised by Young and Faye in 1869 for determining the time of contacts, the author proceeded to show that this was difficult to realize practically, owing to the exact accuracy with which the slit had to be adjusted to the point of contact, and the large size of spectroscope required. He had therefore adopted the simple method of observing the second and third contacts with a small direct-vision spectroscope without a slit. This obviates any exact setting, for the spectroscope can be very small and held in the hand, the observer watching until the continuous band of spectrum narrows down to zero for the point of second contact and until the photospheric spectrum reappears at third contact. This method had been tried during the last total solar eclipse, and the author had succeeded in estimating the instant of second contact to a degree of accuracy which made it possible for the thin stratum of vapours lying close to the photosphere known as the "reversing layer" to be photographed.

Mr. Seabroke. Did you see the reversing layer?

Mr. Shackleton. No, for the moment the continuous spectrum disappeared I gave the signal to "go" and prepared to work the dark slides, for if there was a "reversing layer" it must be visible at that moment, and a photograph was far more important than a visual observation.

Prof. Turner. It is always interesting to know the exact facts of a successful observation. Would Mr. Shackleton kindly tell us exactly what he saw?

Mr. Shackleton. I watched the eclipse for ten minutes before totality began. At that time one could see the chromospheric arcs crossing the band from the photosphere. As the Moon advanced this narrowed down with increasing speed until at length it dwindled to a small thread, and when it got to this I gave the signal to go.

Mr. Dyson then read a paper by *Mr. Davidson* "On an Apparent Error in the Motion of Stars in relation to the adjustment of the Polar Axis of a Telescope." He said that the paper was connected with the adjustment of a photographic equatorial. The fact was the introduction of photography had made it necessary to adjust the equatorial much more accurately. With visual observations it did not much matter whether the polar axis of the telescope was pointing exactly to the pole, but it became of great importance when a photograph of long exposure was to be taken. At first it would seem rather a simple matter to drive the equatorial so as to take photographs. One would imagine that all we have to do is to make the equatorial go round at the rate of sidereal time, but there are two disturbing elements. Consider the case of a star near the meridian: refraction brings it somewhat nearer to the meridian, so the rate of a clock must be altered and slowed down so as to counterbalance this. Further, when an attempt is made to photograph stars near the pole it is exceedingly important that the polar axis should be pointed accurately, otherwise the telescope is turning on a wrong axis, and images of stars, instead of being circular, will be parallel lines. Dr. Rambaut last December gave a table of the corrections for refraction, and Mr. Davidson has added to that a correction for the adjustment of the polar axis. The accurate adjustment of the polar axis of an equatorial is not on the true pole, as at first sight would appear. It should be adjusted on the pole as it is seen affected by refraction; that is to say, it should be 45″ nearer the zenith in these latitudes. If there were a star at the pole it would be seen 45″ nearer the zenith and the polar axis of the telescope must necessarily point to this apparent position. Mr. Davidson very cleverly found this out, partly from observation, for he found the images were not as round as they should be if the observer did not guide the telescope, and after trying various ways of explaining the fact, he finally discovered the true one. He gives in his table the necessary correction in the rate of the clock to correct both for refraction and the adjustment of the polar axis. This correction is in most cases very small.

The President. I have much pleasure in seeing Dr. Rambaut here, and we will offer our congratulations to him on his first appearance since his appointment to the position of our late esteemed and valued friend Mr. Stone.

Dr. Rambaut. I am glad to have the opportunity of expressing my appreciation of Mr. Davidson's work, and I feel sure from what Mr. Dyson has said that it will be an aid to working with the photographic telescope. In work of this kind I found that a great part of the difficulty of driving accurately was due to imperfection in the adjustment of the polar axis, and I was led to investigate how far the adjustment of the polar axis had to be accurately effected. I understand from Mr. Dyson that he intends to adjust the polar axis on the apparent pole instead of on the true pole. I think Prof. Pickering makes the same suggestion in the Harvard College Annals, and I daresay this device will increase the uniformity of motion. The effect of refraction is to cause the apparent movement of the stars to be unequal in different parts of the sky, so that however the polar axis is adjusted, it is necessary to have the clock driving at a particular rate according to the point of the sky to which the telescope is directed. Not having had an opportunity of studying the paper, I am unable to say how far Mr. Davidson's correction will carry out these requirements; but I feel sure that they will simplify matters and lead to good results.

Mr. Dyson exhibited two photographs which had been taken at Greenwich recently with the 30-inch mirror which Dr. Common had made for the new Thompson equatorial at Greenwich. They were taken, he said, on dry collodion plates, and were trial photographs, but were interesting as showing the quality of the new plate which had been used at Greenwich, and also as showing the excellence of the mirror that Dr. Common had made for the Observatory.

Prof. Turner. I trust we may be allowed to express satisfaction that another instrument at Greenwich is being put into going order. We know from some of our Transatlantic friends that the getting of instruments into order is attended with difficulty, and sometimes with danger. I congratulate the Astronomer Royal upon getting through the preliminary stages. With regard to Mr. Davidson's paper, as one who formerly had something to do with Greenwich, I should like to say how gratified I am to see such a work coming from one of the junior members of the staff at Greenwich. It would not be easy to match such an experience from the past history of the place. I am not sure that I quite follow the full result of the paper, but the point seems to be that it is better to point the polar axis to the pole as displaced by refraction. It is a matter of convenience, I suppose, to do that for stars near the pole. As to whether it will turn out to be better for stars all over the sky I am not prepared to say at a moment's notice, but it is a result of a more or less new and startling kind, and we are becoming used to having such little surprises sprung upon us ever since the invention of the dry plate. It *may* have been noticed by others, but I think there is no doubt in this case that it is an independent suggestion.

Mr. Dyson. I think the great point in this adjustment is that it reduces the correction which is required for the clock-driving to an absolute minimum. To get images round and not elongated the driving cannot be constant, but this adjustment makes the variation of rate from sidereal time the smallest possible near the meridian.

The President. We shall all echo the feelings expressed by Prof. Turner. It is a matter of gratification to receive from a member of the junior staff at Greenwich such a suggestion, and one is delighted to see there is a promising young astronomer from whom much more good work may be expected.

Prof. Turner then read a paper by *Dr. Gill* on "The Effect of the Chromatic Dispersion of the Atmosphere on the Parallaxes of α Centauri and β Orionis; and on a Method of Determining its Effect on the Value of the Solar Parallax derived from Heliometer Observations of the Minor Planets." The point raised by Dr. Gill was whether certain systematic errors depended upon the eye not being in a certain direction, or were, as suggested by Dr. Rambaut, due to the difference in the colour of the stars. Dr. Gill had himself discussed his heliometer observations on the former supposition, but Dr. Rambaut had challenged the correctness of this supposition. In the present paper Dr. Gill gave his reasons for adhering to his original opinion. He showed that there was possibly an effect of the kind suggested by Dr. Rambaut, but that if the discussion were properly made the effect was very much smaller in magnitude than that obtained by Dr. Rambaut, and actually in the reverse direction.

Dr. Rambaut. I am glad to have an opportunity of saying a few words on the subject of this paper. Although I have not had an opportunity of studying the figures which Dr. Gill has incorporated in his paper, the subject does not come to me as a complete surprise, as I have heard from Dr. Gill that he could not agree with the conclusions I arrived at in my paper in the M. N. I think Dr. Gill has not represented the question at issue between us exactly in the form I should like to have it put. My object in the paper referred to was not to criticize Dr. Gill's heliometer observations of α Centauri, which I always regarded with the greatest respect, but to establish the necessity of correcting for the effect of a difference in the character of the light of two stars. This question occurred to me some years ago, and I think, when one's attention is directed to the fact, it becomes obvious that there must be some effect on the measures due to the colour of the stars. If there be a difference in the mean refrangibility of the light of the two stars, then the refraction-constant must be different for the two stars. I also discussed a series of micrometer measures of β Cygni and γ Andromedæ made by myself and a series of measures made by our President for the parallax of 61 Cygni; but though I am afraid my correction does not add much to the apparent accuracy of the value obtained by Sir Robert Ball, still I thought all the observations—my own, Sir Robert Ball's, and Dr. Gill's—did support the view that there was possibly a difference in colour between the principal and the comparison stars which had a sensible effect.

Capt. Hills then exhibited some reproductions of photographs of the corona. These were copies of all the photographs of the solar corona that ever had been taken with the exception of one taken in 1870. These had been reproduced all to the same size and are all shown with the solar north pole in the same direction. It is the intention of the Photograph Committee to put these on sale as soon as possible. They are absolutely untouched reproductions. Capt. Hills said that it was very interesting to note from these the evident superiority of the pictures taken on the old-fashioned wet plates. One of the best of the twelve, if not the best, was one taken in 1871 by Mr. Davis. The size of the reproductions shows a picture of the Sun with a diameter of 1½ inches.

The President. The intimation by Capt. Hills that it will be possible for Fellows of the Society to purchase these beautiful pictures is particularly pleasing. We owe a debt of gratitude to the Photograph Committee, who have done so much for us in selecting from the beautiful collection of pictures belonging to the Society those suitable for reproduction, and this series will be a very valuable addition to that selection.

Mr. Shackleton. Which of the photographs have been selected for reproduction?

Capt. Hills. Those which show the corona best. Of those taken in 1893 two have been selected, one taken in Africa and one in Brazil. I think in future it may be advisable to take more than one of each eclipse, that is if the Committee can afford the expense of reproducing more than one.

Dr. Downing. Capt. Hills said that photographs taken by the old wet plate process are the best. I think that the observers of the coming eclipse should take note of that remark, and ought not to lose the opportunity, if possible, of taking photographs on wet plates. Two of the observers at Vadsö in August 1896 were prepared to take pictures by that process, but unfortunately the weather did not enable them to do anything. I hope Capt. Hills's remark will not be lost sight of.

The following papers were announced and partly read:—

T. J. J. See. "On the Nature of the Orbit of γ Lupi."

Lewis Swift. "List No. 4 of Nebulæ discovered at the Lowe Observatory."

John Tebbutt. "Equatorial Comparisons of Uranus with 41 Libræ, and a probable Occultation of the Star by the Planet."

David Gill. "On the Effect of the Chromatic Dispersion of the Atmosphere on the Parallaxes of α Centauri and β Orionis; and on a Method of Determining its Effect on the Value of the Solar Parallax derived from Heliometer Observations of the Minor Planets."

A. Stanley Williams. "The Great Equatorial Current of Jupiter."

G. Johnstone Stoney. "Approximate Ephemeris of the Leonids from 1897 Dec. 24 to 1898 April 8."

Robert Bryant. "On the Effect upon the Moon's Motion of a Hypothetical Change in the Law of Gravitation."

Wm. Shackleton. "A Spectroscopic Method of Determining the Second and Third Contacts during a Total Eclipse of the Sun."

H. F. Newall. "Note on a Result used by Prof. Wadsworth in several recently published Papers."

C. Davidson. "On the Apparent Diurnal Motion of Stars as affected by the Adjustment of the Polar Axis of a Telescope." (Communicated by *the Astronomer Royal.*)

Royal Observatory, Greenwich. "Observations of Comet *b* 1897 (Perrine)."

The following Candidates were proposed for election as Fellows of the Society:—

Capt. E. J. Griffin, Commander of the U.S.S. Co.'s 'Moor,' Lieut. R.N.R. (proposed by Capt. D. Forbes).

J. Nevil Maskelyne, 88 Trinity Road, Upper Tooting, S.W. (proposed by E. W. Maunder).

Ambrose Swasey, Cleveland, Ohio, U.S.A. (proposed by Sir R. S. Ball).

John Vaughan, Sub-Lieut. R.N.R., Commander China Navigation Co., Shanghai, and 42 Aubert Park, Highbury, London, N. (proposed by T. Mackenzie).

Thomas Emley Young, B.A., President of the Institute of Actuaries, 108 Evering Road, Stoke Newington, London (proposed by G. F. Hardy).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE Seventh Annual Meeting of the British Astronomical Association was held at University College, Gower Street, on Wednesday, October 27th, *Mr. N. E. Green, F.R.A.S.*, the President, in the Chair.

The names of 20 Candidates for Membership were read and suspended, and the election of 13 new Members was confirmed.

The report of the Scrutineers of the Ballot for the election of the new Council was then read, and the appointments of the Editor and Directors of Sections were confirmed. The following is a complete list:—*President*, N. E. Green; *Vice-Presidents*, Miss E. Brown, Arthur Cottam, Mrs. A. S. D. R. Maunder, W. H. Wesley; *Treasurer*, W. H. Maw; *Editor*, E. Walter Maunder; *Secretaries*, J. G. Petrie, William Schooling; *Librarian*, F. W. Levander. *Directors of the Observing Sections*: *Sun*, Miss Brown; *Moon*, Walter Goodacre; *Mercury and Venus*, Henry MacEwen; *Mars*, E. M. Antoniadi; *Jupiter*, Rev. W. R. Waugh; *Saturn*, N. E. Green; *Comets*, A. C. D. Crommelin; *Meteors*, Henry Corder; *Star Colours*, G. F. Chambers; *Variable Stars*, J. E. Gore; *Double Stars*, G. M. Seabroke; *Spectroscopy*, J. Evershed; *Photography*, Joseph Lunt. *Other Members of the Council*, Rev. J. M. Bacon, Miss A. M. Clerke, Sir Howard Grubb, Sir Wm. Huggins, Arthur Kennedy, G. J. Newbegin, Dr. Isaac Roberts, David Smart, Dr. G. Johnstone Stoney, F. J. Wardale.

The Treasurer, Mr. Maw, then presented the accounts for the past year, and in doing so called special attention to the very heavy and increasing outlay on the publications, amounting to £590, or 10*s.* 3*d.* per Member in return for a subscription of 10*s.* 6*d.*

Mr. Ellis, one of the Auditors, pointed out that but for the entrance fees and some special donations, the Association would not have been able to pay its way. He also suggested that Members should complete their sets of the publications issued prior to the date of their election.

Mr. Maunder supported Mr. Ellis's suggestion, and added that while Mr. Maw, as Treasurer, wanted to cut down the expense of the Journal, he, as Editor, wanted to spend more money upon it.

The accounts were then unanimously adopted.

Mr. Petrie announced that the next Meeting of the Association would be held at Sion College, on the Thames Embankment, where they would have a beautiful hall in a more accessible locality.

Mr. Maunder announced that the Association had now received a definite offer of a site in the grounds of the Royal Botanic Society for the erection of an observatory. The site was one of the most central and convenient in London, and afforded a good horizon, and it only remained therefore to raise the necessary funds. The cost would probably be between £300 and £400. They were to some extent provided with instruments, the President, Mr. Green, having offered his beautiful 18-inch reflector, Mr. Crawford, of Dollond's, a 2-inch transit-instrument, and Mr. Calver a mirror of any size they wished to have and which he was capable of making.

The President then proceeded to deliver his address. He remarked that there were many things in connection with the British Astronomical Association in which he took an interest, and he had selected a few of them for notice. In the first place there was photography, which would doubtless be the power of the future. Its successes were many and gave promise of almost infinite extension; its failures were few and capable of removal. The grand truthfulness of the general results of photography made any departures from this all-important character the more palpable and painful. How was it that we never got a photograph of Mars, Jupiter, or Saturn with anything like the detail that could be seen with the eye assisted by a good telescope? It was because their light was too feeble to produce an instantaneous image on the plate, while the rotation of the planets made it necessary that the exposure should be exceedingly short. The remedy for this difficulty was to make plates very much more sensitive, and, he might add, by taking a considerable number of them in order that the most perfect might be selected. He had every confidence that this would be attained, and that they would eventually possess photographic pictures of the planets as perfect as any that have been produced of the Moon.

Referring to the planet Mars, the President said that at one time he had not been able to give credence to the existence of those mysterious lines—the canals; but the testimony of so many Members of the Association had thoroughly convinced him at least of their existence, although what he had failed to see twenty years ago at Madeira he could hardly hope now to see for himself. There were, however, two remarks he would like to make on this subject. In the first place he thought a more appropriate name than "canal" might be found for markings that must be at least 50 miles broad to be visible at all, and in the next place he had noticed that those observers who depicted the canals most clearly seemed to fail to see, or at any rate to record, the far more obvious shaded markings.

The President then proceeded to give a most interesting account of the old Selenographical Society, of which he had been an early member and for some time secretary. This Society was formed in 1877, and met by permission of the Royal Astronomical Society in their rooms in Piccadilly. The first President was Mr. W. R. Birt, the Vice-Presidents Messrs. George Knott and the Rev. T. Webb, all now passed away. Mr. Herbert Sadler was Secretary, and the Council consisted of Messrs. James McCance, Ainslie Common, Nathaniel Green, Edmund G. Loder, Edmund Neison, and Capt. Noble. The cessation of the Society was brought about partly by the death of some of its most energetic members, and partly by the state of its finances, the printer's bill having eaten up all the subscriptions.

In conclusion the President suggested that the annual dinner might be made a more popular gathering, or that occasional conversaziones might be held for the purpose of bringing the Members together and making them better acquainted.

Votes of thanks to the President for his address, to the retiring Members of the Council, to the Auditors, and to the Scrutineers were then passed.

Mr. Maunder read a paper by *Mr. Pannekoek*, of Leiden, on "The Existing Drawings of the Milky Way, and the Necessity of Further Researches," which Mr. W. Wesley supported, urging that drawings from naked-eye observations had some advantages over photography for depicting the Milky Way.

An interesting paper on "The Rotation Period of Venus," by *M. Antoniadi*, was then read. As the result of a rigorous and systematic investigation at Juvisy, M. Antoniadi had arrived at the conclusion that the markings most constantly recorded were due simply to contrast, and the more fugitive markings of the planet were of purely atmospheric origin, and that we were not authorized to speak of the rotation period of Venus, whose dazzling atmosphere appeared to hide all true surface-markings from our view.

After a short discussion the Meeting adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The opening Meeting of the Session was held on Wednesday, Nov. 17, at the Institution of Civil Engineers, *Mr. E. Mawley, F.R.H.S.*, President, in the Chair.

Mr. R. H. Curtis gave the results of a comparison between the sunshine records obtained simultaneously from a Campbell-Stokes Burning Recorder and from a Jordan Photographic Recorder.

The Campbell-Stokes Recorder consists of a sphere of glass 4 inches in diameter, supported in a metal zodiacal frame. A card being inserted in one of the grooves according to the season of the year, the Sun, when shining, burns away or chars the surface at the points on which its image successively falls, and so gives a record of the duration of bright sunshine.

The Jordan Recorder consists of a cylindrical box, on the inside of which is placed a sheet of sensitive cyanotype paper. The sunlight, which is admitted into the box by two small apertures, acts on the paper and, travelling over it by reason of the Earth's rotation, leaves a distinct trace of chemical action. In an improved pattern two semi-cylindrical boxes are used, one to contain the morning and the other the afternoon record.

The Campbell-Stokes instrument gives a record of Sun *heat*, and the Jordan instrument a record of Sun *light*; and whilst it is probably true that, as a rule, the burning and chemical effects vary directly with the brightness of the Sun's rays, yet it by no means follows that the conditions which will produce the most active chemical action must necessarily and always be those most favourable for burning. It has been the opinion of most observers that the photographic instruments yield a larger record than those of the burning type. In order to set the matter at rest, the Council of the Royal Meteorological Society determined to institute a comparison between the Campbell-Stokes and the Jordan Recorders, which should thoroughly test the capabilities of the two instruments, and at the same time afford reliable data for determining how far the records yielded by the one may be accepted for comparison with those obtained from the other.

These simultaneous observations were carried out by Mr. E. T. Dowson at Geldeston, near Beccles, and extended over a period of twelve months. The records were sent to Mr. Curtis for tabulation, who gave the results of his examination in this paper. After describing the methods adopted for the measurement of the records, Mr. Curtis drew the following conclusions from the figures:—(1) In the case of the Campbell-Stokes instrument the records are capable of being measured with a very fair degree of accuracy. (2) The records of the Jordan instrument afford room for much greater difference of opinion as to what ought to be tabulated, and consequently measurements of the Jordan curves are open to considerably more doubt than are measurements of the Campbell-Stokes curves. (3) When the whole of the photographic trace which can be distinctly seen, but including portions of it which are decidedly faint, has been carefully measured, the amount will approximate sufficiently to that of the Campbell-Stokes instrument to allow of records obtained from both forms of instrument being compared *inter se.*

From an examination of the records at other stations it appears that on some occasions the instruments have begun to record within 13 minutes after sunrise, and have continued up to 10 minutes before sunset.

Mr. Curtis concluded his paper by calling attention to various defects in the adjustment and working of the instruments, and pointed out how these might be overcome.

After the paper had been read, an interesting discussion ensued as to the merits of the respective sunshine recorders.

The Leonid Display, 1897 *.

Very unfavourable weather appears to have prevented the successful observation of the Leonids at their recent return. In consequence of this the impression seems to have gained ground that the phenomenon did not occur as predicted. This is, however, a mistake. Could those observers who saw so little on the night of the 13th, have viewed the sky late on the following night, they must have been satisfied at the character of the display. Between about 4.30 and 6 A.M. on Monday morning the 15th, the usual streak-leaving meteors from Leo became very numerous, and some of them were unusually brilliant, one, which appeared at 5.25 A.M., being brighter than the Moon. Unfortunately the sky was overcast at the great majority of places at the time when the maximum occurred, and very few reports have come to hand, but they furnish unquestionable evidence as to a plentiful fall of meteors in the few hours preceding sunrise on the 15th.

Before midnight on the 13th, and during the early morning hours of the 14th, meteors were comparatively rare, and may be said to have been more conspicuous by their absence than by their presence. The shower of Leonids was of an extremely meagre character, and such as might occur in an ordinary year with the parent comet near its aphelion. But, apart from the disappointing meteors, the night of the 13th was one of singular beauty. At Bristol the sky partly cleared at 11.30 P.M., and the atmosphere became remarkably transparent. The Moon and stars shone very brightly, and films of white cloud, floating rapidly across the sky, gave it a very picturesque appearance.

From the observations reported it may be well to make a few extracts:—

Mr. S. H. R. Salmon, South Croydon.—Sky perfectly clear November 13, 14^h to 16^h. About seven meteors seen, including four bright ones as under:

h	m	s	
15	3	45	First mag. from Andromeda.
15	28	0	First mag. Leonid.
15	32	45	First mag. Not a Leonid.
15	53	0	=Sirius. Probably a Leonid. It fell in a curved path slightly south of Jupiter, and was a beautiful object.

Rev. S. J. Johnson, Bridport.—Tolerably clear on November 13, between 15^h and 15^h 30^m, but not a single meteor was observed.

Miss Brown, Cirencester.—The sky was perfectly clear on November 13, 11^h 45^m to 13^h 15^m, and the eastern sky was watched from a window facing that quarter, but no meteors were seen.

Mr. W. H. Milligan, Belfast.—Cloudy weather prevented observation except on one night, November 13, 11^h^ to 12^h^ 30^m^, when only one sporadic meteor was recorded; no Leonids.

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* From 'Nature,' 1897 November 25.

D. W. Walton, Kingston-on-Thames.—Sky clear and Moon bright on November 13, $10^h\ 30^m$ to $11^h\ 30^m$, but only one meteor appeared. Partly overcast afterwards, but a few faint meteors were noticed in breaks between the clouds.

Bristol.—The writer watched the sky on November 13, between about $11^h\ 30^m$ and 13^h, but no Leonids were recorded. The sky clouded after 14^h, and the following night was overcast throughout. Mr. Corder, at Bridgwater, reports that the nights following November 13 and 14 were too cloudy to permit of observation.

W. Trueman Tucker, Loughborough.—On November 14 the clouds cleared away between 16^h and $16^h\ 30^m$, but the Moon was very bright, and must have extinguished many of the smaller meteors. Between $16^h\ 30^m$ and 18^h a considerable number of shooting stars were observed, but no exact count was kept. The lines and approximate paths of ten of the more conspicuous ones were noted, and the brightest of all appeared in Cepheus at $17^h\ 25^m$. It was sufficiently luminous to cast distinct shadows in spite of the moonlight. Very brilliant meteors also fell at $17^h\ 10^m$, $17^h\ 15^m$, $17^h\ 35^m$, and $17^h\ 40^m$.

'The Dumfries and Galloway Standard' of November 17 reports that the nights of November 12 and 13 were overcast. On November 14, soon after midnight, the clouds began to roll off, and the sky afterwards became very clear. An amateur observer began watching at $12^h\ 30^m$, and from that time until $16^h\ 15^m$ he noted in all only thirty-three meteors, not more than thirteen or fourteen being Leonids. He then, thinking the outlook not sufficiently promising for any striking development of the shower, folded up his note-book and star-charts and retired. Shortly afterwards, however, the Leonids increased rapidly both in numbers and brilliancy. At about $16^h\ 50^m$ "a couple of young bakers going along the Whitesands to their work were startled by many meteors throwing their fiery lances athwart the sky. They estimate the numbers as not fewer than ten shooting stars to the minute, and state they never saw so many before in all their lives."

The latter description is probably exaggerated, but it seems to convey an expression of fact such as we should expect from persons not acquainted with, but surprised by, an unusual celestial event. Though the rough estimate given as to the number of meteors visible may far exceed the actual figures, there is no doubt from the corroborative testimony afforded by Mr. Tucker at Loughborough, quoted in a previous paragraph, that the Leonid shower displayed quite a special activity on the morning of November 15. That it was apparently observed by few persons is unfortunate, but no other result could be expected in view of the cloud-laden atmosphere which prevailed, generally, at the time. It is to be hoped that more favourable conditions obtained at foreign stations, and that successful observations were secured. The shower seems to have presented itself somewhat later than the time expected, both in 1896 and 1897. W. F. DENNING.

A Determination of Precession *.

AT the Paris Conference, held in 1896 May, Professor Newcomb was requested to re-determine the value of the precessional constant, and in accordance with this request has published a new determination based on the Bradley stars. The determination of this constant is a matter of extreme difficulty. If the stars had no proper motions it is evident that the movement of the pole of the Earth among them would be easily found. If they were all at such a distance that their parallax, due to the motion of the Sun in space, was insensible in a century, the question would be simplified materially. As it is, however, the equations which the observations of a star furnish for the determination of the precession contain both the distance of the star and what Professor Newcomb designates its "motus peculiaris." These are clearly unknown quantities for any individual star, and even when stars are taken collectively we have only rough indications of their average distances from their magnitudes and from their proper motions, and the latter need to be very carefullly used to avoid argument in a circle.

In its relation to the determination of precession the proper motions of the stars must be treated as errors of observation are treated; but unfortunately the proper motions are not distributed according to the least-squares law of error. This is, of course, not to be wondered at any more than the divergences shown in other statistical examples, for instance: "the height of St. Louis school girls aged 8" or "the frequency of enteric fever with age" (see Mr. Karl Pearson, Phil. Trans. 1895, A. Part i. pp. 343-414). But as the distribution of proper motions diverges from the least-square law the application of least-square methods cannot be expected to give a very accurate determination. However, no course essentially different is open, though some modifications may be introduced, such as the rejection of stars of large proper motion. Professor Newcomb uses several different methods, and makes separate determinations from stars of different magnitudes. For those who have not access to Prof. Newcomb's paper, an account of the different methods is given in A. J. 405. The different methods of treatment give sufficiently accordant results, and the stars of different magnitudes agree as well as would be expected, but the correction *derived from Declinations is about* 1″·0

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* By Prof. Simon Newcomb. Papers published for the use of the American Ephemeris.

per century greater than that derived from Right Ascensions. This discordance may be explained in part as due to an error in Prof. Newcomb's new system of fundamental right ascensions which he has used in the discussion. Comparison with the Greenwich Sun observations 1836–1895, assuming that the troublesome personal equation to which Sun observations are unquestionably liable is not systematically different for the modern and the older observers, indicates a correction to Prof. Newcomb's normal system of about +0″·5 per century. The observations of Mercury indicate a correction of +1″·0. Altogether Prof. Newcomb considers that all the evidence points to an error in his normal system of about +0″·3 or +0″·4 per century. This diminishes the difference between the results derived from Right Ascensions and Declinations, and from consideration of these still somewhat discordant results Prof. Newcomb decides to apply a correction of +0″·80 to the General Precession which he adopts in his "Astronomical Constants," and to leave the normal system alone.

	General Precession.
Newcomb (Astr. Constants)	50″·2373
Newcomb (Value of Precession) ..	50 ·2453
Struve-Peters	50 ·2524

It does not seem to us that an adequate reason is adduced for the substitution by astronomers of Prof. Newcomb's new value for that of Struve-Peters. The new value differs as much from the value of the "Astronomical Constants" used in Prof. Newcomb's Planetary Tables as it does from Struve-Peters'. The determination rests almost entirely on Bradley, to which considerable systematic corrections have been applied. The difference between Newcomb and Struve-Peters is only 0″·71, and (quoting Prof. Newcomb) "when a value of the precessional constant is once adopted, it should be continued as long as possible without change." The Struve-Peters value is in general use all over the world, with the exception of France, where Leverrier's value is used—for instance, in the extensive and laborious determinations of proper motion recently made by Auwers, Becker, Porter, Stumpe, Rogers, and others; and till a more definitive result from further discussion of more material can be obtained, the change would not be universally accepted, and confusion would arise. Further, other recent determinations, such as those of M. Ludwig Struve and M. Nyrén, do not agree with Prof. Newcomb's. The value does not harmonize with the normal system of Prof. Newcomb. Further, a new normal system is in preparation by Dr. Auwers, and before making changes it is desirable to see what value the precession would have if obtained with this system.

Though it does not seem to us that Prof. Newcomb has obtained a result of sufficient certainty to make so small a change desirable, we are indebted to him for a very full and interesting discussion, The question is looked at from all sides, and the many difficulties with which the determination is surrounded are carefully considered and discussed. Probably the best result obtainable from the material is obtained. But it seems to us that there is more material, and that shortly still more will be available. The question is opened, we may hope it will not be let drop, and Prof. Newcomb's paper may serve in many ways as a model to be followed in subsequent discussions. F. W. Dyson.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Tuttle's Comets.

Gentlemen,—

May I crave space for a few more words on this subject? partly because Prof. Kreutz has kindly written to me to point out some inadvertencies in my former letter and to call attention to later matters about H. P. Tuttle, and partly because there is something bearing upon it in Prof. Holden's recent 'Memorials' of the Bonds.

Prof. Kreutz refers to Ast. Nach. vol. xlix. col. 141, by which it appears that Comet VII. 1858 was discovered by H. P. Tuttle, and its orbit determined by C. W. Tuttle. Comet III. 1858 was also discovered by H. P. Tuttle on May 2 (Ast. Nach. vol. xlviii. col. 331, and the reference, col. 287, which I gave to the former really refers to the latter).

Prof. Holden informs us that H. P. Tuttle perceived Donati's Comet (VI. 1858) some days before Donati, but it was amongst a number of small nebulæ (like that seen by Pons in 1808, which may have been an earlier appearance of Winnecke's Comet), and before he was able to determine its place with accuracy news arrived that it had been discovered in Europe. H. P. Tuttle was also the discoverer of two small planets—Maia, No. 66, on. 1861 April 9, and Clytie, No. 73, on 1862 April 7. He was one of the independent discoverers of Comet III. 1862, which he saw (Ast. Nach. vol. lviii. col. 115) at Harvard College on July 18, the same day on which it was also detected by Thomas Simons at the Dudley Observatory (Ast. Nach. vol. lix. col. 29); but, as is well known, these observers were anticipated by Prof. Swift, who saw this interesting body (now known to be moving in the same orbit with the Perseid meteoroids) at Marathon, Nova Scotia, on July 15.

Shortly after this H. P. Tuttle removed to Washington, having obtained the appointment of a paymaster, so that henceforward his astronomical observations were those of an amateur. Whether this removal is what Prof. Holden refers to in the letter in the 'Memorials,' dated 1863 July 15, where we are told that circumstances, chiefly arising from the unhappy civil war, deprived the Harvard College Observatory of the services of Mr. Tuttle and some others, or whether this relates to C. W. Tuttle (we in this country cannot forget that a civil war more than two centuries before this deprived English astronomy of a very promising votary on a battle-field), I cannot say. But, remarkably enough, as H. P. Tuttle was an independent discoverer of the comet connected with the Perseids or August meteoroids, so was he afterwards of that connected with the Leonids or mid-November stream. For he noticed Comet I. 1866, at Washington on January 5 (Ast. Nach. vol. lxvi. col. 367), but Tempel had seen it at Marseilles so early as 1865 December 19.

The last we hear of H. P. Tuttle is that he was the first (on January 26) to perceive Encke's Comet at its appearance in 1875, and was with Prof. Holden when he made the first observation obtained of it on that occasion (Ast. Nach. vol. lxxxv. col. 157). It should also be mentioned that he took part in the calculations arranged by the committee for the observation of the transit of Venus in 1874.

I am sure that your readers will, with me, be obliged to Prof. Kreutz for calling my attention to some of the above particulars; and also think we should be glad to be informed of anything further that is known respecting the Tuttles.

Yours faithfully,

Blackheath, 1897, Nov. 9. W. T. LYNN.

GENTLEMEN,—

Mr. Lynn, in his interesting reference to the comets found by the brothers Tuttle, says that it remains uncertain whether the discovery of Comet VII. 1858 was due to H. P. or C. W. Tuttle. The discovery was really effected by H. P. Tuttle, for Mr. W. C. Bond, writing from Harvard College Observatory, Cambridge, U.S., to Sir G. B. Airy, said: "We have obtained the following positions for a comet discovered at this Observatory by Mr. Horace Tuttle on the evening of Sept. 5, 1858" ('Monthly Notices,' vol. xviii. p. 303).

Three comets appear to have been first seen by Tuttle in 1858; but with regard to the one catalogued as III. 1858, I have met with no description giving the initials or christian name of the discoverer. Mr. Bond, in announcing the object to Mr. Carrington, said: "Mr. Tuttle discovered a new comet at this Observatory on the evening of May 2nd." A search in the Ast. Nach. or American scientific publications of the period would probably show H. P. Tuttle to have been the discoverer. Chambers, in his catalogue, gives the surname Tuttle only for this discovery, though he mentions initials in other cases.

With reference to Comet III. 1861 this was undoubtedly found by H. P. Tuttle. The following list appears to comprise all the cometary discoveries of the brothers of this name:—

No.	Date of Discovery.	Discoverer.	Notes.
I. ...	1853 March 8.	C. W. Tuttle.	Discovered by Secchi, March 6.
I. ...	1858 January 4.	H. P. Tuttle.	=Mechain II. 1790.
III. ...	1858 May 2.	— Tuttle.	
VII. ...	1858 September 5.	H. P. Tuttle.	
III. ...	1861 December 28.	H. P. Tuttle.	
III. ...	1862 July 18.	H. P. Tuttle.	Discovered by Swift, July 15. Comet of the Perseids.

H. P. Tuttle, like J. G. Galle (1839–42) and F. De Vico (1844–6), seems to have been very successful, for a short period, as a finder of comets. The former was probably the discoverer of the pretty bright nebula in Draco at R.A. $18^{h}\,23^{m}$, Dec. $+74^{\circ}\,31'$ (N. G. C. 6643), but in the catalogue the name Tuttle is given without initials to show which of the brothers is meant. I have frequently picked up the object in question; it is one which must be familiar to everyone who sweeps for comets in the northern sky.

I was recently interested in noticing how persistently Prof. Encke called the Comet I. 1819 "Pons's Comet" (see, for example, 'Monthly Notices,' vol. xviii. pp. 306, 324, and 326). Though astronomers had universally termed it "Encke's Comet," and it had become known by that title and no other, he still, with characteristic modesty, preferred to distinguish it with the name of the famous comet-hunter of Paris and Marseilles. No one could, however, question the superior merit of Encke in ascertaining the true period of this "Mercury of Comets." Moreover, Pons was but one discoverer of four, for Méchain was really the first to pick up the comet in 1786 January; it was also discovered by Caroline Herschel in 1795 November, and by Thulis in 1805 October. As regards discovery, the priority lay with Méchain, as it did also in the case of H. P. Tuttle's periodical Comet I. 1858, of which we have previously spoken.

Yours sincerely,

Bristol, 1897, Nov. 5. W. F. Denning.

On the Reduction of Observations.

Gentlemen,—

In a letter under the above title, published in your No. 258, Mr. Wadsworth—one of the future Yerkes observers, it seems—criticizes my measures of the Companion to Sirius in a manner which I don't quite understand. He reproaches me because I have given the result of my measures with too many decimals, and his opinion seems to be (taken in consideration the first part of his letter) that my measures have no scientific value.

I am sure that no *astronomer* (for the *public* my measures were *not* made) has deduced from my use of three decimals my pretension that the measures are accurate to 0″·001. My use of three decimals is the natural consequence of the determination of the value of a revolution of our micrometer screw. This value was deduced from *about* 400 *observations* which were affected with a *mean* (not probable) error of ±0″·0024. That is a trifle, considering the great number of observations, and gives me the conviction that the *real* value of R at a temperature of +10° C. does not differ from the observed mean by more than 0″·0024. I think, therefore, I do well in using several decimals in my computations, and in publishing the results with three decimals.

Concerning the accuracy of my measures, let us compare my measures with those of the Lick Observers. My six measures given in No. 254 of the 'Observatory' vary between 3″·590 and 3″·902, consequently by 0″·312; the six measures of the Lick Observers given in No. 3389 of the Astron. Nachr. vary between 3″·57 and 4″·00, consequently by 0″·430. The mean of my measures is 3″·7575, that of the Lick measures 3″·74. The position-angle of my measures varies between 188° 16′ and 190°, with a mean of 189° 8′; that of the Lick measures varies between 188° 18′ and 190° 6′, with a mean of 189° 16′·8. I think such a close agreement (in so *difficult* a matter) between a 36-inch and a 7-inch speaks in favour of our "comparatively small instrument" (as Mr. Wadsworth contemptibly says).

After all, I think that the small size of our instrument is no hindrance to obtain accurate results; besides, *Theory* and *Practice* are two different things: *theoretically*, Mr. Wadsworth attributes to our 7-inch a resolving power of 0″·6; *practically* I resolved 42 Comæ when its distance was but 0″·47. *Theoretically* our refractor should not show stars below 13·3 mag.; *practically* it showed the satellite of Neptune (13·82 mag.), and sometimes even those of Uranus, which are of 14·25–14·41 magnitude.

It may be mentioned that Dembowski (whom Burnham calls the greatest measurer of double stars) made his unrivalled measures with a 7-inch. Yours faithfully,

Lussinpiccolo, 1897, Oct. 20. LEO BRENNER.

Winnecke's Comet.

GENTLEMEN,—

We are soon likely to hear of the reappearance of this comet of short period; and the fact leads us to think a little of its first discovery, the date of which is a little doubtful. That it was detected by Pons on the 12th of June, 1819, about five weeks before its perihelion passage in that year, is certain; but it appears not improbable that the same astronomer and successful comet-discoverer had seen it more than eleven years before, in February 1808. On the 6th of that month, about 4 o'clock in the morning, he noticed a very small comet near the boundary between the constellations Serpens and Libra, moving in a southerly direction; he followed it for three mornings, after which the moonlight became too strong to see it, but could not succeed in determining its position accurately, partly on account of confusion with the great number of nebulæ in that part of the heavens. However, he gave, in a letter to Schumacher (Ast. Nach. vol. vii. col. 113), a diagram of its position with respect to two of these, as noted on the morning of February 9. Oppolzer (A. N. vol. lxxv. col. 107) identified these nebulæ with M. 12 and M. 10, and taking Pons's words in their natural sense, that the diagram represents objects as seen in a reversing telescope, and that the comet was very near the northernmost of the nebulæ (M. 12), concluded from his calculations that this comet was really identical with that which was discovered by Pons himself in 1819, and has, since its re-discovery and the satisfactory determination of its orbit by Prof. Winnecke in 1858, been known by that astronomer's name.

Clausen had suggested in a letter to Olbers (Ast. Nach. vol. x. col. 383) that this comet (1819 III.) was identical with 1766 II., the orbit having undergone considerable change in consequence of perturbations produced by Jupiter. Oppolzer alludes to this in the 'Sitzungsberichte der mathematisch-naturwissenschaftlichen Classe' of the Vienna Imperial Academy of Sciences, lxii. Band, ii. Abtheilung, p. 657, but says it was then (1870) too early to make the comprehensive calculation of the perturbations which would be required to settle the point, and I am not aware that anyone has taken it up since. Prof. Winnecke, alluding to the conjecture in 1858, remarks (Ast. Nach. vol. xlix. col. 118) that the appearance of the comet of 1766 II. was very different from that of 1819 III. and 1858 II.; but the perihelion distance of the first, as calculated by Burckhardt from very defective observations, was only about half that of the others, and one cannot say what effect this might have had upon the appearance of the comet.

We are all familiar with the history of Winnecke's Comet since its re-discovery and the definite ascertainment of its orbit in 1858. Unfavourably placed at the return in 1863, it was next observed in 1869, when it was first seen by Winnecke himself on the 9th of April, and passed its perihelion on the 11th of June. Borrelly was the first to see it in 1875, on the 1st of February, nearly six weeks before its perihelion passage on the 12th of March; on the 24th of April in that year Dr. Julius Schmidt succeeded in perceiving it at Athens with the naked eye. It was due again in the winter of 1880, but escaped observation owing to its unfavourable position. In 1886 Mr. Finlay detected it at the Cape of Good Hope on the 20th of August, and the perihelion passage took place on the 16th of September. The next return was in the summer of 1892, when the comet was detected by Dr. Spitaler at Vienna on the 18th of March and passed its perihelion on the 30th of June. Probably soon after these lines are in type it will once more come into the view of our telescopes. Yours faithfully,

Blackheath, 1897, Nov. 1. W. T. LYNN.

OBSERVATORIES.

[Continued from p. 422.]

BRESLAU.—Magnetic and meteorological observations.

DRESDEN.—With the 12-inch Grubb refractor phenomena of Jupiter's satellites were observed and the various comets which appeared in 1896.

DÜSSELDORF.—Ninety observations of 29 minor planets were made during the year.

GENEVA.—"L'année météorologique qui va de décembre 1895 à fin novembre 1896, avec 1167 millimètres d'eau est l'année la plus humide du xix^{me} siècle à Genève!" Observations of zenith-distance have been made to complete the determination of the latitude. It is noted that the level error has changed 0^s·64 in the year, and the aximuth error 1^s·50. With the great equatorial photographs have been taken of the Moon and of Jupiter. A large number of chronometers have been tested during the year. To the meteorological instruments has been added a Campbell-Stokes Sunshine Recorder.

GÖTTINGEN.—With the great heliometer measures of the equatorial and polar diameters of the Sun were made on 37 days; distances of Jupiter from neighbouring stars near the time of its opposition on 5 nights, of Saturn on 3 nights, and Mars on 5 nights; the diameter of Jupiter on two nights when near opposition, and on two nights near quadrature; the diameter of Mars on four nights; 70 Ophiuchi nine times; measures for the triangulation of the Clusters *h* and χ Persei on three nights. Comets Perrine-Lamp and Swift were also observed and a triangulation of stars near the Pole has been begun. The small heliometer was used for measuring wide doubles on 43 nights. The meridian-circle has been used on 83 nights, principally for lunar and planetary observations.

JENA.—A refractor was devoted to observations of comets and small planets. The transit-circle was only used for time-determinations.

KALOCSA.—The Sun's limb was examined for prominences completely on 164 days, and partially on 30 days. In the early part of the year the Sun's activity was very great; but there was a striking diminution about August. On June 20 there was an eruption in latitude −16°. A prominence on the Sun's limb reached the height of 6′ 46″. On Aug. 25 and 26 there was no prominence of more than 30″. Drawings of sun-spots were made on 160 days. In conjunction with the Professor of Physics at the "Lehranstalt," a determination was made of the length of the seconds pendulum. Incidentally it was found that the Observatory clock, which had formerly been considered only second-rate, when erected on a firmer pier had a remarkably constant rate. A barograph has been added to the meteorological equipment.

KIEL.—Since the death of Dr. Krueger the direction of this Observatory has been temporarily entrusted to Prof. Lamp. A sextant has been procured in which the horizon is determined by means of a rotating top, and experiments have been made with it which show that it can be satisfactorily used on board ship. A Repsold measuring-machine for photography has also been obtained. Two 8-inch object-glasses have been cleaned and refigured. The curious discovery of Comet Perrine-Lamp was made in 1896 January. The observations in the prime vertical for latitude made in 1892–4 were given in Ast. Nach. 3343.

KIEL. *Astronomische Nachrichten.*—In addition to the work of the Centralanstalt, a little progress has been made with the comparison of the Astron. Gesell. +55° to +65° Decl. (Helsingfors) with older catalogues.

KÖNIGSBERG.—At the Repsold Meridian Circle observations of stars between 83° and 85° declination have been carried on, as well as observations of the Sun, major and minor planets. In May a Repsold movable micrometer was introduced for the purpose of eliminating personal equation. The observer moves a screw by hand, keeping the star on a wire, while the head of the screw makes contacts at intervals and registers on the chronograph. This instrument works very satisfactorily. With the heliometer, observations of β Cygni, 6 Cygni, Σ 2398, κ Delphini, and δ Persei have been made for parallax. Measures of diameters of planets have been made for comparison with double-image micrometer measures. Prof. Franz has completed his measures of Struvé's stars between +15° and +30° with the heliometer. He has also measured Lick photographs of the Moon to detect any deviation from the circular form. A new 14-inch refractor has been added to the equipment.

LEIPZIG.—The Zone Observations are ready for press. The parallaxes of Brad. 3077, O. A. (N.) 10,603, and 31 Aquilæ have been determined. Researches are being made on the influence of atmospheric dispersion in the measures of distance between stars of different colours.

LUND.—Zone Observations finished. Observations of 842 stars with large proper motions.

MILAN.—With the 18-inch refractor 483 measures of difficult or quick-moving double stars have been made. With the 8-inch refractor some measures have also been made for comparison with these. The weather has not been sufficiently good to permit of any addition to the observations of Mars previously made by Prof. Schiaparelli. The difference of longitude between Milan and Crea, a station halfway between Milan and Turin, and of importance in geodetic triangulations. In this determination it is of interest to note that Repsold's recording-micrometers were used, and after a little practice made to work successfully.

PUBLICATIONS.

HARVARD ANNALS, VOL. XXVI. PART II.—This volume, recently received from Harvard, is entitled "Miscellaneous Investigations of the Henry Draper Memorial." It may not be inexpedient to quote a short passage from the first chapter, which informs us of the immense scope of the work now being done in memory of Dr. Henry Draper. "Photographs have been taken continuously for several years with four instruments: the 8-inch Draper telescope, the 11-inch Draper telescope, the 8-inch Bache telescope, and the 13-inch Boyden telescope. The first and second of these instruments are mounted at Cambridge, the third and fourth at Arequipa, Peru. . . . . All of the principal investigations are planned to cover the entire sky, the northern photographs being taken at Cambridge, the southern at Arequipa." The investigations referred to are either photographic or spectro-photographic, for all of the telescopes are provided with prisms which may be placed in front of the object-glasses, so that the instruments may be used for spectroscopy if it is so desired.

It was originally intended that this part of the volume should have contained a discussion of the photographs taken with the 8-inch Draper instrument; but as the number of the photographs taken with that telescope is so large—about 2000 a year are taken with each instrument—it was not found advisable to follow out this plan, so that this part contains some comparatively short investigations as illustration of recent work done at Arequipa.

To mention the pictures first. There are some magnificent photographs of star-groups, the Southern Cross, the Milky Way in Sagittarius, the vicinity of η Carinæ, ω Normæ, and others, all on plates covering a square of 10° side, and some photographs of the spectrum of the variable Nova Normæ. These photographs of star-groups have been discussed in a most interesting way; the plates have been divided up by a network of squares, the number of stars in each square counted, and these numbers tabulated with the distance from the centre of the star-group as argument. The main result appears to be, from data furnished by ω Centauri, 47 Tucanæ, and Messier 13, that the rate of decrease of density of the stars as the distance from the centre increases is the same for all clusters, and an empirical curve is deduced with which the original data are in close accordance. Prof. Pickering says:—"If it can be shown that the law here derived applies to the so-called globular clusters in general, much light will be thrown on the formation of these bodies. It may be safely assumed that their form is spherical, since they appear nearly circular in whatever part of the sky they are seen. The increasing density as we approach the centre is real, and not due to the greater distance traversed by the light within the cluster."

Another section of the book is devoted to the "measurement of position," of which no more need be said than that the Harvard College plates are intentionally not measured with more accuracy than will give the nearest minute of arc, or tenth of a minute of time, and that these places are found by measuring with a reticule with respect to standard stars. Another to the "measurements of brightness," which may be summed up briefly by saying that a star's magnitude is determined by comparing the size of its image with that of comparison stars, a scale of brightness for which is assumed, based on the magnitudes given by Argelander. There is a further chapter on the spectra of the stars in the clusters, which we may speak of in detail on a future occasion.

Many details of the branches of work referred to above are given in the volume, which are instructive, and which we should, if space permitted, have liked to have given more fully. There is one useful hint which perhaps deserves mention when so many persons are seeking accurate methods of driving photographic equatorials, namely this:—Prof. Bailey, at Arequipa, found much difficulty in getting circular images when he used a finder telescope as a guider. He assumed this to be due to unequal flexure of the telescope tubes, and remedied it by fixing an eyepiece beside the photographic plate, and used the photographic telescope itself as the guider.

NOTES.

Comet Notes.—'Comptes Rendus' for Oct. 26 contains some interesting observations of the physical appearance of Perrine's Comet of Oct. 16.

On Oct. 18 the coma was 12" in diameter, with a minute stellar nucleus and a tail 1' in length. On Oct. 21 the coma was 30" in diameter, and there was a distinct nucleus of the 12th magnitude. The tail was conspicuous and straight, about 2' in length, in position-angle 199°.

After this the coma became much more ill-defined, though the tail seems to have persisted without much alteration; its position-angle diminished about 6° daily, being 174° on Oct. 25.

On Oct. 28 the comet had become notably fainter, and the nucleus very difficult to see; the direction of the tail was 143°.

No observations of the comet during November are yet to hand; it is probably now very faint.

Ast. Journ. No. 418 (Supplement) contains the following elements, deduced by M. Perrine from Mount Hamilton observations of Oct. 16, 24, 31:—

$$\begin{array}{lll} T = 1897 \text{ Dec. } 8{\cdot}8471 \text{ G.M.T.} & & \\ \omega \ldots\ldots\ldots & 66^\circ\ 5'\ 42'' & \left.\begin{array}{l}\\ \\ \\ \end{array}\right\} 1897{\cdot}0. \\ \Omega \ldots\ldots\ldots & 32\ \ 4\ \ 5 & \\ i \ldots\ldots\ldots & 69\ 37\ 41 & \\ \log q \ldots\ldots & 0{\cdot}13206 & \end{array}$$

Ephemeris for Greenwich midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Dec. 3....	18 9 54	54 21	Dec. 19....	18 4 59	48 8
7....	18 8 5	52 26	23....	18 4 18	47 7
11....	18 6 45	50 47	27....	18 3 40	46 16
15....	18 5 46	49 21	31....	18 3 0	45 34

Brightness on Dec. 31 0·43 of that at discovery. A. C. D. C.

MINOR PLANET NOTES.—A new planet, DK, was discovered by Herr Villiger, of Munich, on Nov. 19. This is the fourth discovery during the present year. A. C. D. C.

ON THE SPECTRA OF THE COLOURED COMPONENTS OF DOUBLE STARS.—The following is the translation of part of an article by Sir William Huggins in 'Comptes Rendus' for 1897, October 11:—

"By a new arrangement of my slit, I have been able to photograph separately the spectra of the components of some double stars. In view of the probable evolution of double stars by the division into two of the primitive mass great interest is attached to the determination of the relative phase of the evolutional period in which each component is found. One can thus compare double systems as individuals or the relative progress of the components of the single system.

"In 1864 I showed by spectroscopic observations of the components of α Herculis and β Cygni that the pronounced colours of these stars are real, although enhanced to the eye by the effect of contrast, and that the explanation of these colours is furnished by examination of their spectra.

"Recent observations of the components of 12 Canum Ven. show spectra which are in both cases very little advanced in spectral order beyond those of white stars; but the small star is a little more advanced than the principal star. One may conclude, then, that the pair was formed relatively late, and that it is younger than the system of γ Leonis, the two components of which give spectra nearly as advanced as that of the Sun. One must not, however, rely too much on such a deduction, whilst one ignores the masses of the two systems, for it must be granted that a star of smaller mass would pass more rapidly through the successive stages of stellar life.

"Let us look at the components of a single system. A beautiful example is presented in β Cygni, of which the components have well-contrasted colours; the principal star shines with a golden yellow, whilst the other shows a pronounced blue tint. Upon the negatives the spectra are well contrasted, but the spectrum of the fainter star, which is of the fifth magnitude only, is of the first type, as that of white stars which we consider young, whilst the spectrum of the principal star of the third magnitude is very far advanced towards the condition of our Sun, but the dark K line is still a little smaller than H.

"How can we explain this anomaly? It should not be forgotten that besides the fundamental conditions of magnitude and temperature, the light of a star may be much modified by the nature and state of the bodies from which the light is emitted, and by the constitution and extent of absorbing atmosphere which it has traversed. It is possible, then, notwithstanding the apparent chemical constitution of two stars, that the bright star may not be the greater, and that one may take the relatively less advanced condition of the faint component as an index of a larger mass."

THE VARIABLE STAR η AQUILÆ.—η Aquilæ is a star whose brightness varies between the limits 3·5 and 4·7 mag. in a period of $7^d\ 4^h$, the maximum occurring $2^d\ 6^h$ after the minimum. M. Belopolsky, of Poulkova, has for some years past had the star under observation with the spectroscope to determine its velocity in the line of sight. He has recently published his conclusion (Mem. del. Soc. d. Spett. Ital. 1897, Part 7) that the change of velocity in the line of sight can be accounted for on the supposition of revolution in an orbit in a period of $7^d\ 4^h$; but this orbit gives times of eclipse of one component by the other, which are inconsistent with the observations of light variation of the star, and as the author says, we must look for some other cause which will harmonize the two kinds of observations. It is remarkable that M. Belopolsky found a similar want of harmony between the variation of velocity and of light in the case of δ Cephei, and promises some more information on the same subject relative to λ Aquilæ.

Dr. W. J. S. Lockyer has also recently been discussing the observations of this star. From an examination of a large quantity of observations of its light variation he deduces a similar mean period and time of maximum to that mentioned above, and also concludes that the epoch of maximum oscillates to and fro to an extent of five hours on either side in 400 periods: the investigation further shows four secondary fluctuations recurring at intervals of 43 hours, which the author, with some boldness, ascribes to the existence of periodic meteor-swarms, in accordance with his father's "Meteoritic Hypothesis."

The November Meteors.—Supplementary to the observations of the Leonid meteors recorded by Mr. Denning in the article in 'Nature' which we have reprinted on page 447, we are pleased to be able to publish the following record kindly communicated by Prof. Doubiago, of the Kasan Observatory:—"The observation of the Leonids this year at Kasan has badly failed. Between November 7 until November 20 we had only a short clear interval on the 14th and a clear night on the 17th. In the clear region around Leo on Nov. 14 from 13^h until $14\frac{1}{2}^h$ (Kasan M.T.) there were no shooting-stars save one luminous meteor, which was observed through the clouds a few minutes before 13^h. On the 14th between $11^h\ 50^m$ and $13^h\ 50^m$ (Kasan M.T.) five observers counted only 28 shooting-stars over the whole sky, and not one of these belonged to the Leonid stream. The Moon and some clouds hindered the observations."

At Greenwich on the night of November 13–14, which was exceptionally fine and bright, the Leonids were watched for by three observers (Messrs. Edney, Beadle, and Davies), and fourteen only were seen in the course of a four hours' watch, of which six appeared to be conformable to the Leonid radiant. Although, on account of the unfavourable position (in Gemini) and great brilliancy of the Moon, only meteors of the larger class could be seen, and further, that the watch was interrupted for about an hour and a half by cloud, it will yet be apparent that the display was of a poor character. Other nights when Leonids might have been expected were cloudy.

The Andromedes were watched for; but the period was generally cloudy. On the evening of November 25, in a partially clear sky, Mr. Marchant observed six meteors, three of which were Andromedes.

Spectrum of a Meteor.—In a recent Harvard College Circular (No. 20), Prof. Pickering announces that a meteor on 1897 June 18 accidentally left its spectrum photographed on a plate exposed in the 8-inch Bache telescope at Arequipa. The spectrum consists of six bright lines, whose intensity varies in different parts of the photograph, thereby showing that the light of the meteor changed as its image passed across the plate. The approximate wave-lengths of these lines are 3954, 4121, 4195, 4344, 4636, and 4857. Comparison of these lines with those in the spectra of certain stars show an important resemblance between meteors and stars having bright lines in their spectra.

In the 'Astronomical Journal,' No. 417, we have an energetic protest from Mr. Innes against delay in the publication of lists of new double stars. Here is his complaint:—"In the Harvard Circular, No. 18, of 29 July last, Prof. Pickering calls attention to a list of double stars found at Arequipa with the 13-inch refractor six years ago. I desire to record a respectful protest against the belated publication of a list of double stars in such a form. Many observers, myself included, have been devoting their time to a search for southern doubles, and we are suddenly confronted with a list of blank numbers published without particulars as to distance, angle, or magnitude of the companion beyond the general statement that the principal star is of the sixth magnitude or brighter, and that the distance is under 30″, and that those stars found in the Catalogues of Herschel and Russell have been excluded. Such vague statements are very apt to be misleading, and are, as a matter of fact, inaccurate in particular instances."

Mr. Innes then takes the Harvard List and points out which are new and also who discovered the others. This, of course, would otherwise only be known to those who keep such catalogues. But to our mind the chief point is in the side issues, where Mr. Innes remarks that he has not taken any notice of pairs over 15″ apart.

We have before drawn attention to our already swollen lists and pointed out the uselessness of adding to their number by including very wide pairs.

It would not be a difficult matter to make an imposing list in a single night of pairs where the separation was 10″ to 20″.

NEW OBSERVATORIES.—M. Victor Nielsen, the well-known amateur astronomer of Copenhagen, has just built himself an observatory as an adjunct to his new Villa "Urania." The equatorial now being constructed by Messrs. Cooke & Sons, of York, to their standard pattern, is to have an object-glass of 10 inches aperture of the Gauss type, which will be the first large objective from Dr. Max Pauly, the head of the new astronomical optical department of the celebrated Zeiss works at Jena (Thüringen). M. Nielsen proposes to continue his old work of making maps of the Moon on a large scale, partly based on the Lick negatives. He will also take photographs by the aid of a doublet magnifier adapted to his equatorial, and, in conjunction with Prof. Dr. Hartwig, the Director of the Bamberg Observatory, will observe variable stars.

We also hear that Dr. M. Seligmann has recently established a new Observatory at Middelfart. His instrument has a 6·4-inch objective by Reinfelder, of Munich. The mounting and clock-work of this is also by Messrs. T. Cooke & Sons.

PROF. SCHAEBERLE last measured the companion of Procyon on Oct. 8. The measures thus stand:—

1896 Nov. 13....	318°·8	4″·59
1897 Oct. 8....	324 ·1	4 ·70

MR. LYNN informs us that Prof. Kreutz has written to him in reference to his letter in our October number on Miss Herschel's comets, that Méchain, speaking of his discovery of Comet 1792 II.

on the 10th of January, 1793, said ('Berliner Ast. Jahrbuch' for 1797, p. 136) that he had been anticipated by Miss Herschel, who had perceived the comet "einige Tage zuvor." There does not appear, however, to exist any record of this, and as Gregory discovered the comet in question on January 8, 1793 (see Mr. Lynn's letter in our volume for 1892, p. 317), it is probable that Méchain, having heard that the comet had been discovered in England, erroneously supposed that this was by Miss Herschel. It seems incredible that, if she had really done so, Maskelyne should not have heard of it when he communicated Gregory's letters to the Royal Society.

It is announced that Prof. H. S. Pritchett, of Washington University, St. Louis, Missouri, has been appointed Superintendent of the U.S. Coast and Geodetic Survey. Prof. Pritchett, who is the son of the Director of the Morrison Observatory, formerly worked at the U.S. Naval Observatory under Asaph Hall, and has had considerable experience in longitude work.

The nearest view of another planet, a small one it is true, but large as such things go, is now to be had in New York, where Lieut. Peary has landed the huge lump of metal, 70 to 90 tons in weight, which is most likely a meteorite. This was reported by Sir J. Ross as long ago as 1818, but was only located by Peary in his arctic explorations about three years ago.

The Sheepshanks Exhibition at Cambridge has been awarded to Messrs. Martin-Jones and Pocock, of Trinity College. Prof. Turner and Mr. F. W. Dyson were the examiners.

The Oxford Note-Book will be continued in subsequent numbers notwithstanding the announcement made below.

EDITORIAL.

At the request of my brother-editors I announce the fact that after this number my name will disappear from the cover. It ought to have done so long ago, for the management of the magazine has been left entirely to them for some years; but their great amiability has always deprecated any change. "We are going along all right, why make any alteration?" has been their position. In justice to them, however, I have at last had to insist that those who do the work should get the credit. There is a good deal of work in editing this magazine, as past and present Editors know. That it is work which we all enjoy on the whole does not prevent its being a care and a burden at times; and it is time that it was made known that for some years past the care and burden have rested (where they will rest, I hope, far into the future, though the form of the wish is open to criticism) on the shoulders of Messrs. Hollis and Lewis.

In leaving the Editorial Chair after 10 years' occupation of it, I may perhaps say a word or two as to the position of the Magazine. It was founded by the present Astronomer Royal in 1877; was taken over by Mr. Maunder in 1882, with whom in 1885 Messrs. Downing and Lewis associated themselves. Dr. Common and myself became Editors in 1888, and when I left Greenwich for Oxford at the end of 1893 the Editorship passed into the hands of Messrs. Hollis and Lewis. During all these years the Magazine has just about paid its way. At one time the business ability of Mr. J. Power (now at the Cape Observatory), who took a share in the business management, seemed to promise a slight margin of profit: but taking one year with another the promise was not maintained. The various Editors have at times anxiously discussed whether they could afford to spend more in illustrations, such as some of their subsidized competitors in America can offer to their readers; but the amount available for such a purpose has always fallen short of their hopes. Perhaps the above-quoted motto of the future Editors is a good one: let well alone. The magazine, we hope, has a distinct place in astronomical literature, and under present circumstances it does not seem easy to do more than is being done. But our readers may like to know that when there is money for a "picture" they get one; and thus there would be more pictures if there were more subscribers. H. H. TURNER.

FROM AN OXFORD NOTE-BOOK.

THE Leonid meteors have fallen, but fallen below expectation; and some of us are probably saying (in good Irish) that "we always thought they would." The daily press has shown an intelligent interest in the matter, but there have been some notable lapses from accuracy; as, for instance, in the 'Daily Telegraph' of November 11. This particular notice seems to be the result of an interview with "Sir Norman Lockyer and his son Dr. W. J. S. Lockyer," for these are the only names which occur in it, and they occur more than once. The old firm with the new partner and titles is apparently bidding for the monopoly of meteors in addition to the corona. Those who have themselves suffered from the interviewer will forgive a good deal to an unfortunate interviewee. Thus the spelling "Temple," the date 1833 for his comet instead of 1866, the inaccuracy of the prediction of its return—these one can overlook as the fault of the interviewer. But it is difficult not to resent such advertisement of quite the wrong names in an article about meteor observation. And where is the humour in the first, and the sense in the second, of the following sentences?

(1) As Dr. Lockyer has humorously observed: "One would like to get into a meteor swarm to see what it is really made of."

(2) In Mexico fixed observations have yielded meteors only; and by 1899 a

similar product may, perhaps, result in this country, though the phenomena may, of course, not be so vivid.

MR. W. F. DENNING kindly sends me the following note:—

> There appears to have been a pretty brisk return of the Leonids on the *morning of November* 15. At Loughborough the sky was clear between 4.30 and 6 A.M., and among other meteors seen by Mr. Trueman Tucker, F.G.S., were five very brilliant ones. One at 5.25 A.M. was sufficiently luminous to throw the observer's shadow on the ground in spite of the moonlight. At Dumfries, about 5 A.M., two young bakers were going to work and express themselves as startled by the number of bright meteors visible. They estimated the rate as 10 *per minute*, but this is probably far in excess of the truth. It is a pity the weather was so overcast on the morning of the 15th in the S. of England.

EXTRACT from the 'Natal Mercury' of 1897 October 22:—

> Dr. McClean, donor at a cost of £14,000 of a telescope to be erected at the Cape Observatory, has been entertained at dinner by the Governor, the party including nearly all the men of light and leading in the Peninsula. Dr. McClean arrived at the Cape in May, and has since devoted himself at the Observatory to completing his great spectroscopic survey, begun at his private observatory near Tunbridge Wells. His work here is now completed, and he sails on Wednesday next. The telescope, which is the handiwork of Sir Howard Grubb, Dublin, is ready for shipment.

The news of this entertainment, of which Dr. Gill has also kindly sent me full particulars, is most pleasing in many ways. It appears that the project originated unofficially with some of Dr. Gill's friends; but Sir Alfred Milner, hearing of it, wished to give the dinner himself, and invited the party to Government House. It was a kindly and significant act, and strengthens one's conviction that the right man has been put in the right place. [This being an Oxford Note-Book I may perhaps remark that Sir Alfred Milner is an actual Fellow of New College, and the news of this courtesy to science was received in the College with special pleasure.] It must have been a great disappointment to Mr. McClean not to see his gift erected at the Cape Observatory, as he seemed to have every right to expect, before he started. But he has at least accomplished the work which he went to do. [*Later.*—I have just heard (Nov. 15) of Mr. McClean's safe return; he had bad weather the last two months of his stay, but in spite of that had managed to complete the survey.]

THE late Director of the Lick Observatory is apparently not to be allowed to retire in peace; and if one-half of the accusations made against him are true, one must admit that he deserves little sympathy. Professor Colton has "preferred a list of charges against him" of which the following head-lines give a picturesque idea. They are taken from the 'San Francisco Chronicle' of October 20:—

> TERRIFIC ROAST FOR CZAR OF THE MOUNTAIN.
>
> FORMAL ACCUSATION IN HOLDEN'S CASE FILED.—THIRTEEN CHARGES AND SPECIFICATIONS TWO SCORE.—COLTON TELLS A LONG STORY OF OBSERVATORY WOES.—FROM DEAD RATS IN THE RESERVOIR TO SCIENTIFIC FAKING THE PROFESSOR'S INDICTMENT RANGES.

The head-lines are followed by a long enough "story of observatory woes." It is alleged that the pollution of the water by dead rats was deliberately suppressed and the water allowed to be drunk. As regards the "scientific faking" the following instance is representative of several quoted:—

> In the year 1892 it was announced that a large quantity of dynamite would be exploded in the Bay of San Francisco, at a certain hour on a certain day. Dr. E. S. Holden adopted measures to ascertain the effect of the explosion at Mount Hamilton. None of the astronomers except Dr. Holden observed any motion. Dr. Holden, however, observed a distinct motion of the Earth coincident with the hour fixed for the explosion. He wrote an account of his observations and conclusions to the newspapers in San Francisco, and sent the same by a messenger to be posted in San José. He learned from the newspapers however, that the explosion had not taken place at the hour fixed, and he thereupon sent a messenger in haste to overtake the bearer of the communication to the newspapers; and the observatory escaped ridicule by a narrow margin.

In a Common Room at Oxford, where a betting-book was kept in days gone by—now preserved in the archives—I was asked the other day whether "Encke's Comet had ever come back." The questioner in glancing through the book had seen an entry which, so far as he remembered, was a bet against the return of Encke's Comet within some thousand years or so. It was, of course, possible that the comet was not rightly identified, and he kindly looked through the book to "verify the reference." Sure enough there was a slip of memory. The bet, made in 1858, stands as follows: "Two shillings to one that Donati's Comet will not reappear within 3000 years." It was doubtless a cynical comment on the announcement of the calculated period (2000 years). If the stakes were put out at compound interest they ought to amount to a good round sum when the 3000 years is up, in spite of future Mr. Goschens; say a sum requiring 30 or 40 figures to express it.

Talking of the return of comets reminds me that there is a sensational astronomical story in one of the Christmas numbers *, which is a fine example of how a few facts can be twisted out of shape in the domain of fiction. Emerson G. Crellin, an American millionaire, locates on the Surrey Hills "the immortal Crellin Reflector with its sixty-four inch object-glass," and offers "the seductive prospect of becoming chief of the Crellin Observatory at an almost dazzling salary, and having the finest telescope in the world absolutely at his disposal," to Arthur Lennox, a man still in the early thirties, who had not only been Gold Medallist at London and Senior Wrangler at Cambridge, but "had so far distinguished himself in original astronomical research that he had gone straight from Cambridge to Greenwich and had made himself one of the most distinguished of the Astronomer Royal's assistants." All this sounds fairly right except the dazzling salary. The American millionaire, however keen on having the largest telescope on Earth, seems to have little or no ambition in respect of the salaries of his assistants. But let that pass. With this "reflector" Mr. A. Lennox discovers a comet "swimming in space far outside the confines of all known systems," and with unusual reserve kept "the discovery a secret until all the calculations of its orbit had been completed." This conduct affords such a contrast to the usual telegraphic scuttle for precedence that one feels quite seriously tempted to regard it as commendable. But let it pass. When the calculations are completed he finds that the comet is going to hit the Earth "precisely thirteen months from today," with such violence as to make an end of it in one of two ways. This news is disclosed to, and verified by, an alarmed world. Even this we may let pass. But now comes the novelty (which is not even wholly a novelty after all; for has not Jules Verne shot the Moon previously?) in the suggestion of how to avert the impending disaster. The comet is to be shot at and knocked into smithereens before it hits the Earth, and this ingenious scheme is actually carried out with a margin for failure which makes one absolutely breathless. When the nucleus is hit, it is only 100 miles from the Earth, and as the projectile itself only left the Earth 10 seconds previously, the threatened collision must have been so imminent as to justify us in regarding the escape as a "near thing"; and it is difficult to see how there would have been room for the Americans to fire the other barrel, as proposed in the story, if the first had missed. As a reward for this brilliant achievement Arthur Lennox receives the daughter of the millionaire in marriage, and a telegram from America including the words "America thanks you. Any terms you like for lectures."

---

* 'Pearson's Weekly': "The Great Crellin Comet."

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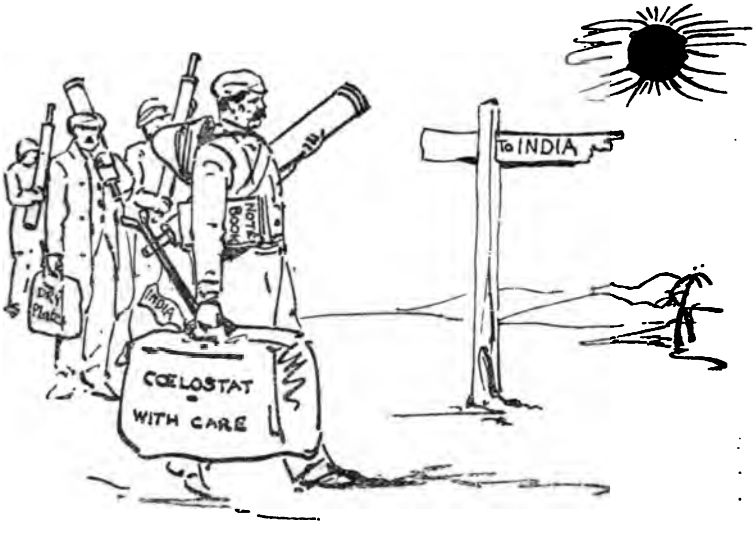

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SAVILIAN PROFESSOR,
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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